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McKellar Stewart was Hughes Professor of Philosophy in the University of Adelaide for twenty-six years. His undergraduate years were spent in Melbourne under Professor Laurie, and after graduation he studied in Edinburgh with A. S. Pringle-Pattison and later in Marburg. He returned to Melbourne as Lecturer in Philosophy from 1912 to 1923.

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PROFESSOR MCKELLAR STEWART AS A PHILOSOPHER

By Sir William Mitchell, K.C.M.G., M.A.

Pringle-Pattison, besides being directly intimate with the German developments, was a clear critic of foundations throughout the history of philosophy. It was with these that Stewart concerned himself, though encouraged by the structure on them which Pringle-Pattison was later to raise in his twenty Gifford lectures on *The Idea of God*. In 1911 he published his *Critical Exposition of Bergson's Philosophy*, taking especially its doctrine of intuition, where insight is from the unity or whole, in contrast with the way of intellect, whose grasp is by analysis, or dissecting. That gave him pleasant reading and an easier task than he undertook twenty years later on the "new science", as Husserl called his *Pure Phenomenology*. The attraction to Stewart was that it brackets the whole known world, and reverses the usual question. The usual question is how mind has been able to make the world known and intelligible. It is because the world is *to* or for experience or consciousness, which therefore has phenomena of its own quite beyond the bounds that psychology sets for itself.

In 1928 W. R. Boyce Gibson spent his vacation from Melbourne at Freiburg with Husserl and the school, and in 1931 published a translation of the best-known treatise. Stewart wrote two articles reviewing it in the *Australasian Journal of Psychology and Philosophy*,

and said that more were to follow. But he undertook instead to write an introductory book for the whole literature. Some years later he had nearly finished it when he woke one morning to find his house on fire with his library and all his manuscripts already destroyed. I think he was glad of the laborious work which the Council asked him to undertake with the coming of the war as assistant to Sir Angas Parsons and then as Vice-Chancellor himself.

After retiring he meant to return to the task and write the book, though in a more convenient way, I think. For the last time he called on me he spoke of some lectures in which Husserl relates his doctrine to that of Hume. It was to Hume that Stewart thought everyone should relate himself, if he would know the reason that is working within him. That is what has recalled to me his belief in the part that philosophy could have in a scheme for all undergraduates, by which before graduating they should have a definite view of the universe. The scheme was approved by the professors after a discussion of schemes for bringing some science into the arts faculty, and some arts into the science faculties. These were condemned on good grounds, but all agreed to assist in the other. The scheme would naturally come into the department of philosophy, and would require its organiser to have the qualifications of a professor. . . . Stewart was keen and confident about the project. He said that his own part would come after the physical world, including life, and would take up the step from nature to mind as in the history of modern philosophy, which began and continued with the history of modern physics. He would bring out the continuity in the one as in the other, notwithstanding its conflicts, and especially its errors, because they reveal the reason working in everyone without question until it is challenged. Several causes prevented the scheme from starting, and the Carnegie Corporation did not mind. It would have supported for its usual period a professor for all students, and said that there is one in several American universities, and that he is professor of Biography!

THE ETHICAL COPULA AGAIN

By Denis Grey

The reason for this critical note on Professor Prior's paper "The Ethical Copula"[1] is that I feel myself to be very muddled about this subject. And it will be worth while to be muddled aloud, if thereby (and as I hope) Prior and perhaps others may be enticed or provoked into further discussion of these very perplexing problems. So perplexing, indeed, do I find them that I am not at all sure that what I am going to say will be sensible. And it may well be that I have understood Prior imperfectly or not at all, and that the criticisms I shall offer are therefore invalid or inept. With this caveat by way of preface, I shall proceed in the convenient form of assured polemic.

Let me begin by stating what I take Prior to have said. I am concerned almost exclusively with the possible 'objective theories of moral modalities' which he suggests after his discussion of the resemblances and differences between 'ought' and ordinary modal words, and with the argument on which these suggestions rest. Of these theories I shall take only the first two in detail; because I think that the weaknesses here vitiate also the second two propounded by Prior. These weaknesses I take to be (a) a false analysis of the logic of inherence-facts, which produces and is produced by (b) a confusion between the predicative, auxiliary and existential senses of the verb 'to be'. These two together lead Prior into forgetting what he himself declares to be the important thing about 'ought', and into suggesting as possible theories theories which are based on an assumed analogy between ought-

[1] This *Journal*, Vol. XXIX, No. 3, December, 1951, pp. 137-154.

relations and the relation known as predication in logic and inherence in metaphysics.

The first theory assumes that "there are facts in which the relation of obligation replaces the relation of inherence" (p. 150, line 3)—inherence, that is, of a characteristic or universal or predicate in a particular or a subject. According to this view, one would hold that the propositions 'John is kind', 'John ought to be kind' express "first-order" facts whose "elements" are John and kindness. What distinguishes the two sorts of fact is the different way in which these same elements are combined. The fact that John is kind is a fact of straight inherence in John of the characteristic 'kindness'. The copula 'is' represents, in a broad sense of the word, the "form of unity" of the fact—what Whitehead would call the mode of ingression of this universal into this particular (or, if we look at it the other way round, the mode of the universal's reception by this particular). It does not represent an element, but the way in which the elements mutually combine, and in combination are, or produce, the fact. Similarly with the fact that John ought to be kind. What is represented by 'ought to be' is a different mode of union or form of unity. On this view 'ought to be' is a copula in precisely the same sense as that in which 'is' is a copula.

When we turn to the kindred facts about kindness, viz. that kindness inheres in or 'is present in' John and that kindness is obligatory upon John, we are dealing with "second-order" facts. They are second-order facts because they have as one of their elements the form of unity of the corresponding first-order fact. And in these second-order facts the forms of unity are identical: they are expressed in both cases by the predicative copula 'is', representing straight inherence. Their elements differ because the two first-order forms of unity differ. And so, in the proposition 'Kindness is obligatory upon John', the modal 'ought' has disappeared from the copula (where it is in 'John ought to be kind') and turns up in the complex predicate qualifying kindness, viz. 'obligatory upon John'. We

have replaced a relational proposition by a predicative proposition containing the appropriate relational predicate. Or as Prior puts it (p. 151, line 21) 'no matter what may be the form of unity you start with, you can always find an equivalent fact of a higher order in which the form of unity is inherence or absence'. For convenience I will call this view view A.

Prior next proceeds to sketch a modification of view A, which I will call view B. I understand it as follows: Supposing we say that 'ought' is a subject-attribute relation (as before) but add that of subject-attribute relations only predication (representing inherence or non-inherence) expresses a first-order form of unity. Then it follows that 'ought' expressions express only second-order facts, *i.e.* facts containing as an element first-order forms of unity. I have not put view B as Prior puts it, because I find his language ambiguous, and it seems best to deal with the ambiguities critically: I have therefore removed them (I hope) from the exposition of view B.

Let me turn now to criticism—not so much of these views themselves, as of what seems to be implied in the way in which Prior puts them forward. He does not, I think, incline to either of them himself; but he must regard them as possible or plausible, or he would suggest only to dismiss.

Consider first view B. The conclusion which Prior actually draws as appropriate for this view is not that 'ought' expressions express only second-order facts, but that all 'ought' expressions are, or should be made to be, predicative. Now the premiss of this view asserts that, *of subject-attribute relations,* only predication expresses the form of unity of a first-order fact. It does not assert that all first-order facts are inherence-facts—that is to say, it leaves open the possibility that some first-order facts are properly expressible otherwise than by subject-attribute relations. Nor does the premiss assert that only predicative propositions will be allowed to express second-order facts—that is to say, it leaves open the possibility that there are second-order facts which are not inherence-facts. But the conclusion Prior draws on this view is the following: "The

grammatical form of the proposition 'John ought to be kind' is . . misleading; the fact that it expresses is really the second-order fact that kindness is obligatory upon John".

It seems to me that if this is to be his conclusion, Prior must be adopting one of the following three positions:

i. He might hold that although in the case of first-order facts predication expresses inherence, in the case of second-order facts it may express other forms of unity as well. That would be compatible with the conclusion. But it involves holding that there are other forms of second-order facts besides inherence-facts. And in that case, 'John ought to be kind' could express a second-order fact as it stands. Nothing is to be gained by changing it to 'Kindness is obligatory upon John', and there is nothing in the argument of view B to justify the change.

ii. He might hold that there are other first-order forms of unity besides the inherence expressed by predicative propositions. But in this case the conclusion is nullified. For 'John ought to be kind' might then express a first-order fact, and we have no warrant for saying that obligatoriness is a subject-attribute relation.

iii. He might hold that all first-order facts are inherence-facts, and are properly expressed only by predicative propositions. It would then follow that all second-order facts are inherence facts also, and of the kind required. This seems to me the only cogent form of view B. But I cannot think that Prior would regard it as plausible—or that if he did, he would propound it in language so oblique. For in this form, view B simply amounts to asserting the metaphysical dogma that all facts are inherence-facts, and that predication is the only proper expression of them. And such a view leaves no room for "moral modality"— there is no first-order fact from which a predicate 'obligatory upon' could be extracted. Nor can we conjure out of thin air a relational universal 'obligatoriness', while we banish from the realm of fact the relation from which it is derived. Either we must admit real relations of

obligation, and abandon the dogma, or we must remove from 'ought' any metaphysical foundation. And in that case the logical thing to do is to relegate the moral 'ought' to some convenient positivistic limbo. There is besides a curious consequence to this form of view B, if we take Prior seriously in what he says about the distinction between first- and second-order facts (p. 149, line 17 seqq.). It would appear that whatever 'John ought to be kind' expresses, it is not a fact about John; and indeed that no Englishman has ever had the means of talking about anyone's moral obligations. Though the hope would still remain that we might bring up our children—and pursue the study of ethics—by recourse to some happier and foreign tongue.

Part of the difficulty I find here is due to the "various dodges" which Prior frankly adopts in order to avoid a discussion of qualities and relations which would take him too far afield. And I apologise if I attack him when he is dodging legitimately. But I think the difficulties are also of another kind. Though we are led pretty clearly to believe that Prior makes a distinction between 'inherence' and the subject-attribute relation of predication which expresses inherence, he does not draw a similar implicit distinction between the ought-relation and its logical correlate. Thus it is very hard to tell, for example, what is meant when he says that 'obligatoriness' is to be included (for the purposes of view B) among subject-attribute relations. Is 'obligatoriness' to be ranked with inherence? But that is not a subject-attribute relation, but a relation between a universal and a particular (*cf.* p. 148, line 31). Is it then to be ranked with predication? But that does not carry automatically the conclusion that the relation expressed or represented by 'obligatoriness' is a form of inherence. Yet at times Prior seems to be making just this assumption; and it looks as if the reason for it is to be found in the fact that sometimes he does not distinguish metaphysics from logic. For example, by saying "We might hold that . . . inherence and absence are the only *relations between attributes*

and subjects which can *constitute forms of unity of facts*" (p. 151, line 28) he identifies inherence and predication, which are elsewhere distinct, and so paves the way for the assumption that facts of obligation must be construed as inherence-facts, because we are supposing that 'obligatoriness' is a sort of predication. However this may be, it seems to me that view B is admissible only in the form of a metaphysical theory which cannot admit either facts of obligation or relational predicates like 'obligatory'.

But view B contains a further difficulty, and one which it shares with view A, to which I will now turn. View A seems to be in better case than the other; it allows first-order facts of obligation, and suggests a straightforward parallel between these and facts of inherence. 'Ought to be' appears as a straightforward modal copula—it remains to be discovered, of course, what sort of mode 'ought to be' is of 'is'. But the parallel is sustained by what seems to be a strange discussion of inherence-facts and the expression of them in propositions (p. 148, para. 2; p. 149). Prior clearly takes inherence as a relation, and as clearly holds that what expresses it is a relation, viz. between subject and attribute. But he makes a determined attempt to treat the proposition 'A inheres in B' as a predicative proposition. It is done by translating 'inheres in' by 'is present in'. The next step is to forget that 'John is kind' is a *disguised relational* proposition, which can be otherwise put by saying 'Kindness inheres in John'. Having thus identified the relation of inherence with the predicative proposition (which is treated in logic for some reason as if it were non-relational), we proceed to assimilate the relational 'Kindness inheres in John' to the ostensibly predicative 'Kindness is present-in-John'. And so, says Prior, whereas 'John is kind' expresses a fact about John, 'Kindness is present in John' expresses similarly a fact about kindness. But since it contains inherence as an element (not "named", but "referred to" by the complex predicate present-in-John), Prior calls it a "second-order fact".

This genesis of the distinction between first- and second-order facts, which is exploited in both view A and view B, explains why Prior chooses this distinction between them and no other. For we should naturally expect that second-order facts are those containing first-order *facts,* not forms of unity, as their elements; or are those involving first-order facts as second-order forms of unity. But what Prior is after all the time is to ensure some abiding place for "the peculiar logical importance that undoubtedly does attach to the copulæ 'is' and 'is not' " (p. 151, line 20). I shall try to suggest presently a different view of the importance of these copulæ.

What has happened is that two facts of different orders have been created out of one fact of a single order. We have a fact, aRb: because of the nature of R—inherence in Prior's sense is asymmetrical—we cannot predicate of a the complex relational characteristic we attach to b. We can say 'John is kind', or 'John participates in kindness', but not 'Kindness is John' or 'participates in John'; 'Kindness inheres in John', but not 'John inheres in kindness'. But is this any reason for saying that aRb and $b\breve{R}a$ represent two facts and not one? And can we find any justification for saying that $b\breve{R}a$ contains the "form of unity" of aRb as one of its *two elements*? The form of unity, if it is not simply a way of saying that there is a fact aRb, must be R. And "element" can only mean a term of the relation. I conclude that Prior's analysis is false, and the four theories which employ it impossible.

But it is on this analysis that the analogy between 'ought' and 'is' is constructed. We are told on p. 151, for example, that the elements of the fact that kindness is present in John are 'kindness' and 'present in John' or 'inhering in John': similarly, the elements of the fact that kindness is obligatory upon John are 'kindness' and 'being obligatory upon John'. "The form of unity in both cases is the inherence-form indicated by the ordinary affirmative copula 'is' " (p. 151, line 11 seqq.). That is to say, Prior is construing 'X is present in Y' as 'X is . . . present-in-Y'. And the 'is' of 'is obligatory

upon' is taken, as before, as a predicative copula. So we can treat 'ought to be' as modal of 'is'. But surely the truth is far otherwise.

I suspect that Prior would never have made this mistake had he thought of putting his examples into Latin or Greek—or indeed almost any language, probably, other than English. For the truth is that in these locutions 'is' is not predicative at all. In 'X is present in Y', 'is present in' is Prior's own translation for 'inheres in'; and his analysis depends on the irrelevant fact that in English there is no single-word equivalent for *adest, inest,* πάρεστι, *occupe,* se trouve, and the rest. There is no occult metaphysical reason why, in 'Kindness is present in John', "the relation of inherence is not named but is referred to" by what Prior calls the *adjective* 'present' (p. 149, line 27). It is simply that he has chosen to replace the present tense 'inheres in' by the present tense 'is present in'. And in this locution 'is' is an auxiliary verb. This particular present tense happens to be formed on the analogy of the English iterative form of verb tenses; we say indifferently, or according to the emphasis we wish to give, 'inheres' or 'is inhering'. But we do not assert a predication in one case and something quite different in the other. We do not assert a predication at all. Thus 'present' in this phrase is not so much an adjective, as an adjectival form—that is, a participle—of a *verb*: and the whole verb or copula is 'is-present-in'.

When we turn to 'ought' and its synonyms, the position is somewhat more complicated. For 'ought to be', though slightly odd if we use it unmodified in its proper sense, is a grammatical and logical unit; and so we tend to use it as if it were a grammatical deontic voice of the verb 'to be'—as Hebrew verbs, for instance, have a causative voice 'to cause to be'. But there is only one sense of the verb 'to be' which is a unit in this way, I think: and in this sense it is neither an auxiliary nor a predicative. It is the existential sense of 'to be', which in Greek you can conveniently distinguish by its special form of accentuation, but which takes more spotting in

English. We rarely use it; and hardly ever with the auxiliary 'ought'. But two suitable persons who took their situation seriously might say intelligently and even with truth 'X ought to be', and proceed accordingly to bring him into existence. The predicative, auxiliary and existential uses of the verb 'to be' are distinguishable and should not be confused. But it is not always easy to tell the precise force or meaning of the word, even when we can distinguish its usage, and I shall not pursue these ramifications further.

Instead I shall suggest that in 'X ought to be kind' and the like, 'to-be-kind' is one verb, as in 'X ought to love'. (In fact I think I am going further than this, and taking 'ought-to-be-kind' and 'ought-to-love' as single complex verbs; but that, if it be so, is a matter of grammar and syntax rather than of logic or metaphysics.) That is to say, if 'ought' be modal, it is modal of a verb. And I greatly doubt whether the predicative 'to be', as in 'X is white', is a verb in this sense. Some languages, like Greek, have a proper and single verb for 'is white'; and indeed the 4th century Greek positivists and semanticists deprecated locutions like 'is white' just because they were so misleading. We should not class predication as an obvious action; but it is worth noting, all the same, that 'X is white' and so forth appear to be 'dispositional' statements which are general references to a class of individual goings-on or actions. And perhaps if we understand the predicative 'is' in this way, we may rank it as a verb proper. In any case, what I want to stress is the invariable connection of 'ought' with a doing or a coming into existence of some kind. Prior seems inclined to hold, toward the end of his paper, that ought-relations hold between subjects and attributes—though I am not sure whether these and their relations are logical or ontological entities. But on p. 138, para. 2, he makes exactly my point. "I am wanting to emphasise", he says, "what a very peculiar relation it (the word 'ought') appears to signify . . . It signifies rather a relation between an object and something that that object can be thought of as doing." If this is so, it

will not be very satisfactory to write it off as signifying a relation between a subject and an attribute—to treat it as if it governed adjectives or their abstract nouns rather than as governing verbs. But I think Prior's earlier point is at the heart of the matter.

I shall not defend this suggestion further than to notice an inconsiderable portion of the linguistic evidence which goes to support it. This evidence of language, however one is to use it, is clearly of great importance. And so far as I know, no one has collected it or examined it adequately. Unfortunately I cannot do more than give a few hints.

Let us consider first the connection of 'ought' with doing, as it appears in the grammar and syntax of 'ought'. In a good many languages, the force of 'ought' in its various gradations is regularly carried by subjunctive or optative inflections (or their equivalent) of the main verb to which 'ought' would be auxiliary. Exactly what or which gradations are so expressed, and whether they vary from one language to another, I have not the knowledge to say. But in English, for instance, we can use 'We ought to say this', 'We should say this', and even (sometimes) 'We would say this' indifferently. Greek usage allows us to say τοῦτο χρῆ λέγειν or τοῦτο λέγοιμεν ἄν. Again in Latin, 'you ought to have held your tongue' can be *oportuit tacere* or simply *tacuisses*. There are complications in all this, owing to the fact that in English 'ought' is tenseless—though even here, there is the colloquial 'didn't ought' or 'hadn't ought' —and has no other mood, except again for the colloquial 'shouldn't ought' and its variants. So that English must use periphrases to achieve the effects which other languages can get by inflections of the 'ought' verb itself; we use 'have' with compulsive force, as a past or future of 'must' and 'ought' ('I had to do it', *sc.* because I knew I (then) ought), or 'it was necessary, right, proper' and the like. It is perhaps worth noting that in Hebrew, or at any rate in modern Hebrew, which shares this deficiency with English, the regular locutions are identical. I am not sure what is the force of these com-

plications. In French, for example, 'il faudrait dire' can be replaced by 'on dirait' as well as by 'on doit (devrait) constater'; but I doubt whether it is possible to replace a straight 'il faut' or 'on doit' by an optative or a subjunctive. However that may be, it does seem that the force of 'ought' is regularly conveyed by the verb of doing itself. And incidentally it is pretty clear that if language is any fair clue to thought, the ethical 'ought' and the logical 'ought' are closely allied; it would seem hopeless to try to keep necessity out of ethics. Again, 'ought' would seem to be connected with hypotheticals on the one hand, and imperatives on the other. Though it cannot be identified with either, we cannot ignore its affinities with both.

There is also a different type of indication that the force of 'ought' is verbal. For in some languages, and languages not cognate, there is a special form of the verb to convey it. Latin has its gerunds and gerundives, and Greek verbs possess similar inflections. It is worth noting that these have to be rendered in English by an ostensible infinitive: *amandum,* 'a loving', say the grammars; but the word is untranslatable in English, and one only gets to it in the other form, *amandus,* 'to be loved'. Locutions like *faciendum est* suggest perhaps that in 'I ought to do' the verbal form 'to do' is not infinitival at all, but gerundive. 'Ought' would then have its proper etymological sense, and the phrase would say as well as mean 'I owe a doing' (*cf.* the Hebrew 'it is upon me to do'). On this view *hoc faciendum est* and 'This is to be done' are syntactically identical. The Japanese equivalent of this verbal form is very interesting and very odd. *Kore wo subeki koto da,* one would say (in one style) for 'I ought to do this', which is literally and as near as English can get to it 'There is a to-be-done thing', *subeki koto.* But the suffix *-beki, -beshi* can be added to adjectives, and it then turns them into verbs. And more than that, this verbal suffix has the force not merely of 'ought', but of the future tense generally, providing that what is being indicated is some sort of compulsion or necessity felt

about this future, and not merely a desiderative force. I would hesitate to build an argument on a Japanese locution, and in any case I am not sure how far the argument from language will take one, however modish it may be at present. But there seems to be a clear indication here that we should do well, at least as a working hypothesis, to regard 'ought' as a verbal mode, and to disregard the fact that in many languages it is also an auxiliary verb.

So far I have been considering the way in which 'ought' may disappear grammatically into its main verb, as an illustration (so far as it goes) of the thesis that 'ought' is essentially connected with doing or the coming into being of new facts. There is another indication of this, or of something very like it. And that is, that in a great many languages one of the root meanings of 'ought' expressions is a sense of lack or need. The Latin *oportet* appears to derive from this, together with the cognate *opus est*—an expression which conflates beautifully the notions of lack and of doing. The French *falloir* has the same root meaning—it derives from *fallere* and *faillir*. The Greek τὸ δέον is connected with this same meaning, though other forms of it are alleged to belong to a second group of 'ought' meanings which I will mention in a moment. And it is justifiable to take the Japanese *-beshi* in a similar sense, since it seems to be connected with the feeling that the important thing about the future is that it isn't here now. To all these, of course, may be added the huge group of *besoin, bisogno* expressions with their parallels and cognates.

The second main sense of 'ought' is the meaning of legally binding or owing: 'ought' itself, *debet* with its cognate French and Italian derivatives, 'obliged' and its cognates, the German 'sollen', the Greek δεῖ (according to some scholars) and so forth. One thinks in this connection of the typical ambiguities of 'right' in its sense of 'owing' or 'due', and the way in which these shade off into 'correct', 'justified', 'proper', 'necessary'. And there is probably some connection also with the 'ought' of causal inferences ('That new piston ring ought to do the trick')

and of quasi-causal situations ('I ought to get the next bus, if I'm not to be late', 'You oughtn't to double the third in a major chord'). For in Western philosophy at least the original sense of 'cause' seems to be a legal one (*αἰτία*, *causa*). In morals, this gives the familiar compulsive nuance of 'ought' which includes both guilt and necessity; for inference in general, the feeling seems to be that we have a right to conclude that q is so, if p is so—whatever the connection between them—that we shan't be let down if we do, that we are justified in so doing.

The third sense is that of 'suitable' or 'fitting', 'right' in this sense—*decet*, *convenable*, Greek *πρέπει*, *ἁρμόττει* and a host of synonyms. Indeed it is this sense which is the key to Greek philosophy from the 5th century to the time of Plotinus; it is also the mathematical necessity of Kepler and Newton, and perhaps of Leibniz. We need hardly remind ourselves of what it has produced in English ethical theory.

I would not wish to base an argument about the nature of 'ought' upon such summary evidence, so scrappily adduced. At the same time, it would be absurd to proceed with such an argument in ignorance of these facts, or by dismissing them *a priori* as irrelevant—especially when we remember that in the very process of arguing we employ language which is partly given to us, and partly made by us. However, I will venture a suggestion, as an alternative to what I take Prior's suggestion to be. His view, as I read him, would perhaps be that in logic we should take 'ought' as a modification of predication—*i.e.* as a subject-attribute relation; and that what the word 'ought' expresses is also a relation, namely a modification of the relation of inherence.

My suggestion is rather different. I will assume that the equivocal position of 'ought' in grammar reflects pretty faithfully the state of affairs it refers to. If 'ought' were a real verb, or at least a real transitive or reflexive verb, then it *would* express a relation: it would be parallel to 'to love' and similar verbs. Though in strictness we should point out that even these verbs do not *express* relations, except obliquely;

what they do express is rather the bringing into existence of a relational state of affairs, and the maintaining of it in existence when it is so produced. But 'ought' is not a verb of this kind. Nor is it a verb like 'exist', which refers even more obliquely to relational states of affairs. Nor, again, is it a verb like 'sing' or 'do'; nor like the predicative 'is', if we grant it the status of a verb proper. It does not express an acting; nor does it quite express an active dispositional state, like 'is kind'. The fact that it is auxiliary to a verb suggests that it is a modification of that verb. And there is some reason to suppose that it is not a verb at all—not even an auxiliary verb—but simply a mood or voice or inflection of a verb. We might then be inclined to suppose that it is a modification of the relation or relations expressed by such verbs. It is this sort of approach, I imagine, which leads Prior to say that the relation expressed by 'ought' involves "abstract inherences" among its terms (p. 153, line 15).

So far the suggestion is not that 'ought' expresses a relation, but that it points to a modification of the relation referred to by the verb it governs or alters. But it is not quite so simple as this. For in 'X ought to love Y', the relations between X and Y which are to be modified are those already existing. X may love Y already, or he may not. But when we say he ought to, we seem to mean, in the main, one of two things. Either (a) we presume that as things are now he doesn't love Y, and we express a conviction which we have, and which X or Y or both should have, that things as they are must be altered, because as they are they aren't 'right'. Or (b) we disregard the fact that X does love Y already, as things are now; and we treat this state of affairs as a potentiality, whether or not it may have been made actual, and find it good. 'X does love Y. Does he? Splendid; he ought to.' Thus it is not 'loving' which is to be modified, or considered as modifiable, but the state of affairs as it is now. 'Ought' points to the *bringing into existence* of a *new* state of affairs, of new relations between X and Y, which—whether or not it actually

has occurred or will occur—is satisfactory in a way in which the old state of affairs is not.

But 'ought' points to more than this. It points also to the fact that X or Y or somebody finds the present or the old state of affairs *lacking* in something, and feels *compelled* to act (or get X to act) so as to fill the gap and to make harmonious and 'right' what was lacking before. So that if 'ought' expresses the modification of any relation, it is the relation (whatever it may be) between the present state of affairs and possible new states of affairs. And 'modification' would have to refer to the fact that of all the potentialities, only a particular one is *the* one, is possible. (It is, to be sure, a very curious sense of 'possible'. But this is not the place to try and make it more explicit.) Yet it hardly seems appropriate to talk of 'ought' as expressing a modification of any relation in this sense. 'Ought' would seem rather to refer almost to a by-product of the fact of bare potentiality, as this fact affects human action. It is not that the *doing* is itself modified; rather, the mere fact that doing is possible carries an 'ought' with it. 'Can' in this sense implies 'ought', and not the other way round.

Thus (the view would run) 'ought' does not express the relation between the actual fact and the potential new fact, nor yet a modification of it. But, granted it refers to a concomitant of the fact of the possibility of action, perhaps we could say it expresses a *characteristic* or quality of this relation? Not quite, I think. For 'ought' faces several ways at once. On the one hand, it points to this relation between actual and potential; but again, it points not to a characteristic of this relation, but rather of the person contemplating the potentiality—namely, that he is affected by it compulsively. And again, it points back-handedly to the *status quo,* which this person finds defective. So that if 'ought' expresses a characteristic, this must be as it were a vector characteristic, which belongs exclusively neither to the agent, nor to the present situation, nor to the projected action, but somehow results from the juxtaposition of all three. In fact Plato may be nearest the truth in holding that 'ought' is our name for

felt necessity of whatever kind or degree. We can feel such necessity as a direct pull on ourselves, or we can feel it vicariously as affecting other people ('X ought to love Y'), or again as affecting other things ('If this is so, then that ought to be so'). But—no feeling of necessity, no 'ought'; merely an impenetrable fact of change. If on such a view we are to use the slippery word 'mode', I suppose 'ought' would refer to a mode of human existence, where 'existence' means 'doing' or 'acting'. At the same time, it is plain that 'ought' is not a name for anything, any more than a tense-inflection or the imperative mood of a verb are names for anything.

My suggestion, then, is about what 'ought' is not rather than what it is. Neither in grammar nor in logic is it to be treated as a relation, since it is some sort of verbal inflection, in between tense inflections and the imperative mood. Nor does it express a relation, nor yet the modification of a relation—and certainly not of the relation referred to by the verb it alters. As soon as we try to *make* it express a relation, we have either to force language and talk about the 'ought-relation', as I have been doing, or we have to talk about something else—the relation of 'compulsion' or of 'obligation'. And neither of these on its own is really what 'ought' refers to. It would look as if formal logic, at any rate, cannot deal with it at all. In the first place, it is intimately connected with desire and with the future. And even if we widen this future reference, and consider it as potentiality in general, the formal mechanism of logical alternatives will not help. In the second place, 'ought' expressions seem to indicate in part our reaction to the world, and in part the nature of the world itself as we experience it and act on it and in it. Logic may deal with necessary relations, but 'ought' refers (or often refers) to the *effect* of such relations when they are *recognised* as necessary. And it is always connected with doing, whether the doing be an action in the narrow sense, or—as with inference—doing in the sense of judging or asserting. But further than this I confess I cannot go—if indeed I have managed to get as far. Can anybody help?

LOGIC AND SINGULAR PROPOSITIONS

By A. J. Baker

It is customary to adopt one of two quite opposed accounts of statements which refer to single persons or things. On the one hand is the view that these statements can be satisfactorily treated as falling under the A, E, I and O forms, in particular as a species of universal proposition; on the other hand, this reduction is replaced by a sharp antithesis, as in the view that law-statements and statements of fact have totally different logical functions. I want to argue that each of these views is guilty of over-emphasis: there are important analogies between the logical roles of singular and universal statements; nevertheless these analogies do not justify the conclusion that singular statements are reducible to propositions of the four forms.

While it is normally conceded that the terms of singular and universal propositions are distributed in the same way, logical relations are often pointed to as an apparent source of difference. For "Socrates is not wise" appears to contradict "Socrates is wise", so that contrary relation does not occur—nor do subaltern and subcontrary relations. But, as was recognised by J. N. Keynes,[1] these relations are implied by our distinction between "always" and "sometimes". Reviewing the life of Socrates we can quite naturally ask whether he was always, never or only sometimes, handsome, ill, snub-nosed or wise. To bring out the parallel with universal statements we can introduce a plural formulation by replacing "always", "never" and "sometimes" by "all instances of", "no instances of" and "some instances of" respectively. Thus "All instances

[1] *Formal Logic*, 4th edition, p. 116.

of Socrates are wise" has as its strict contradictory "Some instances of Socrates are not wise", and similarly with the E and I forms. In other words, we recognise that there are various instances of Socrates as much as of men, even though the former are never in different places at the one time, but instead form a continuous history of Socrates. As a result, we can introduce I and O forms for singular statements and so have all the usual logical relations, immediate inferences and syllogistic inferences.[2]

To assimilate singular to universal statements to this extent is to deny the sharp contrast often made between them. Professor Ryle, for example, has argued that while singular statements have the job of stating facts the job of law-statements is quite different. "Law-statements are true or false but they do not state truths or falsehoods of the same type as those asserted by the statements of fact to which they apply or are supposed to apply. They have different jobs. . . . A law is used as, so to speak, an inference-ticket (a season ticket) which licenses its possessors to move from asserting factual statements to asserting other factual statements."[3] But given the fact that singular statements have instances, *i.e.* are themselves general, it follows that they can equally be inference-licences or major premises in arguments. Just as laws have their singular instances, or matters of fact, coming under them, so singular statements can have their instances, or further matters of fact, coming under them. As a result we can have arguments like "John Smith is an economist, that person talking near the door is John Smith, therefore the person talking near the door is an economist". And while for practical purposes we do not normally make separate statements about what occurs in short periods of time, it is logically possible to continue indefinitely the process of finding

[2] *Cf.* T. A. Rose, Critical Notice of P. F. Strawson's *Introduction to Logical Theory*, this *Journal*, May, 1953. For a general defence of the four forms see also John Anderson's "Hypotheticals", this *Journal*, May, 1952.

[3] *The Concept of Mind*, p. 121.

instances which come under each successive singular statement. Thus "The man who has been talking near the door all the morning is an economist" licences the inference from "The man talking near the door for the last ten minutes is the man who has been talking there all the morning" to the conclusion that he is an economist, and likewise with shorter periods. It follows that singular statements can also be inference licences, so that Ryle has not presented a conclusive distinction between them and law-statements.

There is, however, a further complication in Ryle's argument. We might take his metaphor "inference-licence" to mean simply "major premise", but in a later account he has indicated that he does not regard inference-licences as premises at all; they are instead the "principles" of their arguments. It may be suggested, he says, that "an argument 'p, so q' is always invalid unless the premiss from which 'q' is drawn incorporates not only 'p' but also 'if p, then q'. 'q' follows neither from 'if p, then q' by itself, nor from 'p' by itself, but only from the conjunction 'p and (if p, then q)'. But this notoriously will not do. For, suppose it did. Then a critic might ask to be satisfied that 'q' was legitimately drawn from 'p and (if p, then q)'; and, to be satisfied, he would have to be assured that 'if (p and (if p, then q)), then q'. So this new hypothetical would have to be incorporated as a third component of the conjunctive premiss, and so on forever—as the Tortoise proved to Achilles."[4]

If this view were correct it would also follow that singular statements, since they function like major premises in relation to their instances, are not premises of the arguments in which they appear to occur. But in any case I do not think that Ryle's argument really upsets the ordinary view that the hypothetical (or equivalent law-statement) is a premise and not the principle of an inference. Thus, his view implies that all hypothetical and syllogistic arguments are really forms of

[4] "'If', 'So' and 'Because'", in *Philosophical Analysis*, edited by Max Black, pp. 327-328.

immediate inference—from p to q. But it is clear that he does not mean that all inferences of the form "p therefore q" are valid. For this would imply that every hypothetical, true or false, is an inference-licence, or, what comes to the same thing, that a true proposition entails any proposition, for whatever p and q were, we could always construct the inference-licence, "if p then q". Ryle's view is, therefore, that some arguments of this kind are valid and some invalid, depending on whether the inference-licence is true or false. But the difficulty is that we should constantly have to appeal to the licence for justification. Suppose someone asks: Why is "The demand is increasing, therefore the price will rise" valid, while "The price is rising, therefore the demand will increase" invalid? The answer must be: Because "If the demand increases the price will rise" is true, but the converse is not. So if the hypothetical were the *principle* of the argument we should be proceeding in the same way as those who attempt to justify a syllogism in Barbara by appealing to the truth of the *Dictum de omni et nullo*; in either case a regress is generated. In the case of the syllogism we do not, in fact, have to appeal to the *Dictum* in this way; as it is said, the *Dictum* refers to the "form" of the argument, and we simply recognise that the premises entail (or, in other cases, do not entail) the conclusion. But with the supposed argument, "p, therefore q", we do not recognise as a "logical truth" that "if p then q" is the form of the argument, or the principle in accordance with which it proceeds. In every case, before we could understand whether the argument was valid or invalid, we should first have to answer the empirical question: Is "if p then q" true or false? But since the argument does not, as would follow, go on for ever, the conclusion to be drawn is that we appeal to the hypothetical, not in this way as a justifying principle, but because it is as much part of the argument as the premise "p". Once this is admitted the regress indicated by Ryle does not arise. There is no question of justifying "p, therefore q", because this is not an argument until the second, major premise is taken

as part of the argument. Accordingly, the reference to "inference-licences" can be accepted only as a way of distinguishing the role of major premises from that of minor premises.

We may thus object to the logical separation of singular and universal statements by emphasising the important fact that the former have generality and can be major premises. But, granting these likenesses, it seems to me to be simply an error in the opposite direction to go on from this to infer, as is done by upholders of the four forms, that there are no residual differences.

Consider, as one source of difference, the introduction of expressions like "always" or "instances of Socrates" to reveal the generality of singular statements. Since they have generality in this way it follows that universal statements have a parallel *double* generality, for we may equally make use of expressions like "always" in expressing universal statements. There is a clear difference in meaning between "All men are sometimes tired" (or "Some instances of all men—*i.e.* every man—are tired") and "All men are always tired" (or "All instances of all men are tired"). If we take account of the various differences we arrive at eight possibilities:

All X are always Y	All X are never Y
All X are sometimes Y	All X are sometimes not Y
Some X are always Y	Some X are never Y
Some X are sometimes Y	Some X are sometimes not Y

But we can't carry out the same expansion with singular statements. We can't in a corresponding way say, for example, "All instances of Socrates are sometimes tired", *i.e.* "Some instances of all instances of Socrates are tired" (or if, by assigning a definite time interval to the first order instances, it were held that we could speak in this way, we could then make a further distinction with universal statements by saying *e.g.* "Some instances of all instances of some men are tired" and so still be a step ahead).

This difference is reflected by differences in the verification and falsification of universal and singular statements, *i.e.* by differences in their logical relations. Consider, as an example of the first affirmative form above, (a) All crows are (always) black. Suppose there is a crow called Jack. We can now have as a more general singular statement (b) Jack is black, and as a less general singular statement (c) Today's instance of Jack is black. We now have two ways of verifying (a); what we can call a "stronger" verification by (b) (when (b) is taken with the second verifying statement "Jack is a crow"; I neglect this complication in the other cases) and a "weaker" verification by (c), but (b) is verified only by (c). In other words, however far we carried the process of obtaining more and more specific singular statements, (a) compared with (b) would have one extra type of verifying statement, *i.e.* is at a higher level of generality, or, we could say, is an inference-licence of a higher order. The less general forms reveal analogous differences. "All men are mortal", for instance, may be taken as an example of the second negative form, for it may be readily expressed as (d) All men are sometimes not alive. Socrates is mortal can then be expressed as (e) Some instances of Socrates are not alive. There is then a difference in verification, for to know that Socrates at a certain time has died establishes (e), but the truth of (e) only verifies (d). On the other hand, falsification, if possible, would be very difficult in either case. To falsify (d) we have to falsify a statement like (e) about some given man, but to falsify (e) we should have to have an indefinite series of statements, This instance of Socrates is alive, That instance of Socrates is alive, and so on.

These examples should be sufficient to reveal persisting logical differences. Temporal quantification of singular statements may be introduced to show that they can usefully be worded according to the four forms. But if this is used as an argument to show that their logic is the same as that of ordinary A, E, I and O statements, the difficulty is that the

latter statements may be expanded in precisely the same way. In this case the four forms become eight forms, four of which do not apply to singular statements.

A further difficulty for the logic of the four forms is the singularity of singular statements, both of the simple sort containing proper names and of the sort containing definite descriptions. When the plural translation of "Socrates is wise" as "All instances of Socrates are wise" is made, if the parallel with the A form is to be justified, it has to be contended that the reference to a single person is logically irrelevant, that it concerns the "matter", not the "form", of what is asserted. But if this view is strictly adhered to, the purely material difference between singular and plural statements should be expressed in giving the logical form of the original. The re-statement should include a reference to the fact that Socrates is one person, so that as well as "All instances of Socrates are wise" we should have a further statement, say, that all instances of Socrates belong to one individual, or have a spatio-temporal continuity. There should, that is, be a treatment of singularity similar to that in Russell's theory of descriptions. According to that theory, part of the logical form of "The author of *Waverley* was Scotch" consists of (1) At least one person wrote *Waverley* and (2) At most one person wrote *Waverley*. Now a rigorous defence of the four forms involves upholding the existential import of universal terms as a condition of their intelligible use, so on that account Russell's statement (1) would be rejected as not part of the logical form. I am suggesting, however, that a consistent reduction of singular statements involves accepting statement (2), or some equivalent. But, as Mr. Strawson has argued,[5] statement (2) as much as (1) fails to distinguish what is presupposed by a statement from what is stated by it. That the author of *Waverley* exists or is one

[5] "On Referring", *Mind*, July, 1950; also in *Introduction to Logical Theory*.

person is not part of what the speaker asserts to be true, though he has to assume these things in order to make a genuine statement. Hence it is incorrect to regard either assumption as part of the issue or logical form.

In some uses of singular descriptions the issue may concern the oneness of the terms. The difference between "Nicholas Breakspear was the English Pope" and "Nicholas Breakspear was the only English Pope" is that use of the former presupposes but does not state that there was just one English pope, whereas in the second case this is certainly asserted as something that could be disputed. Similarly, to say "Shaw was not the author of *Hamlet*" is simply to presuppose a singular predicate, whereas to say "Moses was not the only author of the *Bible*" is to state that the predicate is not singular. But the first two statements would provide further difficulties for an account of descriptions according to the four forms. For the difference in meaning would presumably have to be ignored by offering as at least part of the logical form the same two A propositions in each case, *viz.* "All Nicholas Breakspears are English popes" and "All English popes are Nicholas Breakspears". These formulations, it can be observed, would introduce an ambiguity in the plural formulation of singular statements. For to say "All instances of Nicholas Breakspear are English popes" would be to make a false statement; if the "instances" formulation were used it would have to be said instead that "All instances of Nicholas Breakspear are instances of *a man who* was an English pope".

Returning, however, to ordinary singular statements, the statement about oneness in the analysis of "Socrates is wise" is equally not part of the *issue* presented by the original statement. If it were, the original could be denied by denying either of its two component statements. But if someone could correctly object that Socrates was not one person (if, say, the name "Socrates" really referred to some racial group) we would not regard this as *contradicting* the assertion that

Socrates is wise. If the objection were justified the speaker could only withdraw his statement, it would be shown that he hadn't made a genuine statement.

Accordingly, the singularity of terms has to be treated in the same way as their existential import. For if singularity is treated as part of what is asserted the objections to this are of the same kind as the objections to treating the existence of terms as extra stated pieces of information. On the other hand, if singularity, like existential import, is granted to be something merely presupposed, then this introduces a distinction between what is presupposed and what is asserted by singular statements which prevents them from being reduced to plural statements.

Relational statements introduce a further class of singular statements which certainly appear to have special logical properties of their own. But here, too, attempts have been made to reduce them to traditional forms. I want to refer to the most plausible of the attempted reductions but to argue that it does not succeed.

There are, in a wide sense, many universal relational statements, if we count those in which one or both terms really refers to a relation, *e.g.* "All descendants of X are living in America", "All survivors are well". But the normal relational form is "A r B"—the form taken in specifically relational inferences—and here, the most natural and most common way in which we specify "A" and "B" is by referring to single individuals. (It can be noticed that statements containing definite descriptions[6] are really included in relational statements. For the former appear always to have relational terms and so to be what I have just called relational statements in a wide sense; while statements of the special kind combining proper names and definite descriptions, *e.g.* "Shakespeare was the author of *Hamlet*", are examples of the "A r B" form.)

[6] Of the sort which refer to single things, *e.g.* "The American President is a Republican", as distinct from those like "The horse is a quadruped".

These singular relational statements, taken separately, may to some extent be treated as coming under the four forms. In some cases it would be possible to introduce the plural formulation "All instances of A have r to B", and so on, to show that these statements have I and O forms. It is clear, however, that there are differences from other singular statements, for in many cases, *e.g.* descendant of, square of, the particular forms would not arise, while in the case of temporal relations, *e.g.* "John is older than William", the introduction of "instances" would interfere with the meaning. But the important issue concerns the role of these statements in arguments. The typical relational argument, *e.g.* A is north of B, B is north of C, therefore A is north of C, consists peculiarly of singular statements. The question is whether these arguments can be reduced to syllogisms, *i.e.* to arguments containing universal premises.

The case for reduction is again based on the argument that the special features of relations depend on their matter, not their form. The form, A r B, B r C, therefore A r C, is clearly not the form of a valid argument, for we get different results when we replace "r" by "north of" and by "father of". Accordingly, to know whether an argument of this sort is valid or invalid we have to know what is meant by the relation in question. But, it is maintained, to know this is to know something purely material, which, when taken account of, allows us to express relational arguments as syllogisms. Traditional logicians used to carry out the actual reduction by introducing as a major premise what was simply a statement that the argument in question was valid. But the most plausible reduction proceeds by maintaining that relational statements themselves can be reduced to the four forms. The essential step is the transition from, for example, "A is north of B" (for short, A n B) to some universal formulation such as "All things (or places) north of A are north of B" (for short, All nA are nB), together with "Some things north of B are not north of A" (for short, Some nB are not nA). As a result,

the argument: A is north of B, B is north of C, therefore A is north of C, can be translated as follows:

A n B = (a) All nA are nB and (b) Some nB are not nA
B n C = (c) All nB are nC and (d) Some nC are not nB
A n C = (e) All nA are nC and (f) Some nC are not nA

The reduction then proceeds by obtaining two syllogisms which prove the two required conclusions: (1) (a) with (c) proves (e), and (2) either (a) with (d) or (c) with (b) proves (f). There are a number of variants of this type of reduction but the same general considerations apply to them all.[7]

It can be observed that a consistent attempt at reduction would have also to deal with relational immediate inferences like A equals B, therefore B equals A. Either they have to be dismissed as really not inferences (in which case surely the same must be said of *e.g.* No A are B, therefore No B are A) or they have to be reduced to ordinary immediate inferences. Parallels can be offered in some cases; for instance, A is north of B, therefore B is south of A, might be treated as a case of contraposition by taking "B is south of A" as expressed by the contrapositives of statements (a) and (b) in the table above. A somewhat similar account might be given of A equals B, therefore B equals A. But this kind of parallel depends on the fact that these relations are transitive. In many other cases, *e.g.* A is the father of B, therefore B is a child of A; A is the twin of B, therefore B is the twin of A; there does not appear to be even a plausible parallel.

An immediate objection made to the syllogistic reduction is that the crucial step from "B is north of C" to "All things north of B are north of C" assumes the transitivity of the relation and so begs the question whether A is north of C.[8] But this charge can, I think, be avoided if it is maintained that simply because of our material knowledge of the state of affairs described by "B is north of C" we must know that "All

[7] *Cf.* A. N. Prior's "Argument *a Fortiori*", *Analysis*, January, 1949.

[8] *Cf.* Strawson, *Introduction to Logical Theory*, p. 208.

things north of B are north of C" is true. To assert the latter is then not already to assert that A is north of C, any more than to assert that all men are mortal is to *assert* that Socrates is mortal. What is, however, open to attack is the underlying assumption that the singular relational statement must be reduced to a universal statement. This assumption must be made by anyone who insists that the relational argument *must* be reduced to a syllogism. He cannot merely say that the universal statement happens also to be true so that there happens to be a parallel valid argument, for he would then be admitting that the original argument is also valid as it stands; he has to maintain that we cannot understand what is meant by "B is north of C" until it is reduced to its real logical form. In this case we have a form of reductionism reminiscent in some ways of early positivism. The "ultimate" statements are now above all universal statements, but the meaning of certain statements is identified with their truth-conditions—"B is north of C" means what must be true if it is true. But the fact is that its meaning is quite different. Not only can the original statement be understood as it stands (otherwise, why should the translation come as a surprise to some people?) but the translation is not really a translation of it. For, "B is north of C" is a statement which refers to an individual B, while the translation is not only in the plural but refers to a class of things, "things north of B", in which B is not even included.

In a certain sense this criticism is ineffective. For the attempt at reduction arises, no doubt, from the assumption that the form of syllogisms (and of immediate inferences) supplemented by concise rules of distribution, *must* be the model for valid arguments. Given this assumption, differences of meaning can then be treated as irrelevant and the desired reductions achieved, in a way which is logically irrefutable, just because the assumption is *a priori*. But, if our conception of form is less inflexible, we may perfectly well say that we recognise the validity of relational arguments in their existing

singular form. As a way of expressing the form of transitive arguments we can, if we wish, as Strawson has suggested,[9] write: "A r-trans B and B r-trans C" entails "A r-trans C", thus indicating that r is transitive. It is true that before we can say that a given argument has this form we shall have to know that its premises have the form "A r-trans B" and not, say, the form "A r-intrans B". But this does not show that the form of the argument must be syllogistic, for we had to be able to make precisely the same distinction before we could carry out the supposed syllogistic reduction. What it does show is that we cannot speak of relational form in quite the same way as we can speak of syllogistic form. For the way in which we indicate the form of a syllogism is by distinguishing logical words or constants like "all" and "no" from term-words or variables, and this is a distinction already presented by actual syllogistic arguments (for even though we rarely follow the wording of the four forms, differences of quantity in particular usually are, and in any case can naturally be, separately expressed). But relational arguments, as they actually occur, do not refer to the transitivity of their relations; the relation word, so to speak, *always* has partly the function of a variable and partly the function of a logical word, so that when as logicians we go on to make the distinction explicit by writing "A r-trans B" etc., we are only in an extended sense giving the "real form" of the argument. Nevertheless, we are in this way able to give an account of valid and invalid relational forms which is based on the original singular form of relational statements and which does not require us to introduce a strained account of the meaning of those statements.

Finally, I want to refer briefly to the view that singular hypothetical statements are reducible to traditional forms. These statements are of various kinds, but consider those dealing with causal situations, *e.g.* "If the Treasurer reduces

[9] *Op. cit.* p. 53.

taxation, inflation will be renewed", "If Chiang Kai-shek invades the mainland he will be defeated". Professor Anderson, in defending the four forms, has offered an account of hypotheticals of this type according to which we are to regard them as really arguments with understood universal premises.[10] We have to find a "field" in which antecedent and consequent are connected, and when we do so we can make the understood premise explicit. Given "If A then B" we find a field F such that all F which are A are B; the antecedent gives us: this is an F which is A; and from these premises the conclusion, this is B, follows. Leaving aside other features of this analysis, the question I wish to raise is whether we *have* to supply the field and the understood premise in order to give the meaning or logical form of the original statement. Argument about whether either of the above hypotheticals is true or false would no doubt involve argument about whether some universal statement (or set of universal statements) concerning economic or military affairs is true or false, and to carry on such argument would be to advance enquiry. But it is one thing to conclude from this merely that there are logical *analogies* between these hypotheticals and syllogisms, and a quite different thing to go on to support a rigorous reductionism by insisting that to state the hypothetical is really to state the syllogism. For it is quite clear that there would have to be preliminary argument to establish just what the universal premise is. But, in this case, if knowing the meaning of the hypothetical statement involves knowing the universal premise, it appears that many people who make these statements do not understand what they are saying. On the other hand, if they do understand what they are saying the universal premise cannot be an essential part of the meaning of the hypothetical. In other words, statements like those in question show that there are some sorts of saying whose function is to be indefinite and inexact. But this is obscured by the reductionist position which, as in the case of relational statements, involves a

[10] *Op. cit.* pp. 10-15.

reduction to the universal statements which, it is held, must be true if the original statements are true.

To sum up: while it is wrong to treat singular statements as if they had nothing in common with universal statements they are nevertheless irreducible. There are important analogies between singular and A, E, I and O statements. Ordinary singular statements are not purely particular, but have instances which come under them, and so can function as major premises and have all the usual logical relations. There are also parallels between their role in relational inferences and as hypotheticals, and the role of universal statements in syllogisms. Nevertheless, the attempt to reduce them to the traditional forms is mistaken; the various sorts of singular statement are just as ultimate as the forms to which they are supposed to be reduced.

ARGUMENT FROM CHANCES

By Quentin Gibson

1. When we produce evidence in support of the statements we make about particular things or events, the principle on which we argue is sometimes a universal law. When I look at my watch and say "The eggs will be cooked by now", my argument depends on the acceptance of the universal proposition that boiled eggs always take $3\frac{1}{2}$ minutes to cook. In such a case, the application of the law to the particular instance is automatic; to establish the law is at the same time to justify the inference. Hence the crucial step in such arguments has quite naturally been taken to be the establishing of the law.

Very frequently, however, our arguments rely not on an appeal to what always happens, but on an appeal to what usually happens. When I look at the sunset and say "It will be hot tomorrow", this does not mean I am assuming that hot days always follow such sunsets, but only that they do so as a general rule. The obvious regularities in nature are hardly ever without exceptions, and it is to the obvious regularities that we most commonly refer.

But in this case it is clear that the application of the rule to the particular instance is far from being automatic, if for no other reason than that the particular instance may always be one of the exceptions allowed for in the rule. Even when the rule has been established as holding in a reasonable proportion of cases, there is still a crucial step to follow. It is this step in the argument, in which we apply such rules to particular cases, that I propose to examine in this paper.

When we say that something usually happens in certain circumstances, this can be put another way by saying that

there is a good chance that it will happen in these circumstances. This serves to bring out the point that such statements are members of a somewhat wider class which I will refer to generally as "rules of chance". By a rule of chance I mean a statement to the effect that anything of a certain sort, say α, has some more or less accurately specified chance of having some character, say β; or that in certain circumstances, α, there is some chance that an event of a certain sort, β, will occur. Such statements are sometimes called "probability rules",[1] sometimes "proportional generalisations" or "relative frequency generalisations",[2] but the phrase "rule of chance" seems, if anything, to have fewer misleading implications. Furthermore, it emphasises the point that it is this type of statement the formal logic of which is dealt with in what is commonly referred to as the calculus of chances.[3]

The chances asserted in such rules may vary from very high to very low. They may be measured on a scale extending between 0 and 1, though it is rarely possible to state them as precise fractions. The evidence in their support is commonly of a statistical kind, and this is why they are sometimes called "statistical generalisations". It is when the chances are stated somewhat vaguely to be towards the upper, or the lower, end of the scale that we speak of making approximate, or incomplete, generalisations, or of saying what generally, or usually, happens. Though we may regard such statements as only just falling short of laws, we have to recognise that they differ only in degree from the more precise statements of more even chances, such as "There is a 51% chance that any baby born will be a boy".

2. Let us then state our problem as one about the application of rules of chance to particular cases. We will take, as

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[1] *E.g.* by Kneale, *Probability and Induction*, p. 118.

[2] *Cf.* Strawson, *Introduction to Logical Theory*, pp. 239-240.

[3] Whether "chance" is to be *defined* in terms of relative frequency is a further question which is not raised in this paper.

the general form of a rule of chance "The chance that any α is β is p", where p is a fraction between 0 and 1. For short we may write this "C(α, β) = p".[4] The question then is: given that C(α, β) = p, and that a particular thing, A, is α, in what way does this provide evidence for or against a statement of the form "A is β"?

It is clear that the argument—C(α, β) = p and A is α, hence A is (or is not) β—suffers from two defects, as compared with the straightforward application of an empirical law. In the first place, as has been pointed out, however high (or low) the value of p, we cannot conclude that A is (or is not) β, without recognising that A may be one of the exceptions admitted in the rule. Though there is a good chance of a trade-unionist voting Labour, we cannot conclude that Mr. Smith, who is a trade-unionist, will do so, without recognising that he may be one of those who will not. And in the second place, A may always possess characters other than α, in virtue of which it will fall under a further rule in which the value of p is quite different. Thus A may be γ as well as α, and though C(α, β) = $\frac{3}{4}$, say, C($\alpha\gamma$, β) may equal only $\frac{1}{4}$. If Mr. Smith is not only a trade-unionist but also has a personal dislike for the Labour candidate, this will, as we commonly put it, "reduce the chances" of his voting Labour. Whereas, if it were the case that every trade-unionist voted Labour, it would make no difference whatsoever to our argument what other characteristics Mr. Smith possessed.

In order to meet the situation in which these two defects are present, we may formulate two principles:

(1) The value which the rule C(α, β) = p possesses, as evidence for the statement that A is (or is not) β, increases as p varies from $\frac{1}{2}$ upwards (or downwards).

(2) The evidential value of the rule increases the greater the number of characteristics of A it takes into account.

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[4] The symbolism is that used by Kneale, *op. cit.*, save that the letter C is used to conform to the use of the word "chance".

These principles seem clearly to be presupposed by the judgments we commonly make about particular cases. Let us consider them in turn.

3. The first principle is the ground on which we accept approximate generalisations as being "nearly as good" as empirical laws, while refusing to draw conclusions about what will happen when the chances are more or less even. There has seemed so obvious a correspondence between the increase in the chances stated in a rule and increase in the evidential value of the rule that it has often led to a confusion between the chance of any α being β and the likelihood on the evidence that some particular instance of α will be β. Though the identification is a mistake, it bears witness to the ease with which we accept the principle which asserts their connection. Given the principle, we may base on it the recommendation that wherever we have to rely on considerations of chance, we should try to use rules in which the chances stated are as high as possible.

Yet, despite its apparent obviousness, this principle has sometimes had doubt thrown upon it. Such doubt is thrown by people who query whether statistics have any value for predicting what will happen in individual cases. Even if we know that 70 out of every 100 trade-unionists vote Labour, it may be argued that there are so many special circumstances involved in any individual case that it is impossible to tell whether the case will be one of the 70 or not. If we knew more about Mr. Smith, we might discover some law from which we could deduce that he would not vote Labour. Or at any rate we might arrive at some rule of chance which would make it very unlikely that he would do so. Are we not then merely guessing when we say that he will?[5]

The trouble with this line of argument is, firstly, that if we are demanding a law from which we may deduce a conclusion, we will very rarely find one, and hence will very rarely be able to make any predictions at all. This would be decidedly

[5] *Cf.* Passmore—"Prediction and Scientific Law", in this *Journal,* Vol. XXIV, Nos. 1-2, September, 1946, pp. 14-17, for argument on these lines.

alarming, and would be a grave challenge to scientific common sense, which assumes that we can estimate the likelihood of events occurring even when we cannot get beyond approximate generalisations. If, on the other hand, we are merely demanding some further rule of chance which takes account of as many features as possible of the case under examination, we do not avoid the difficulty. The attempt, in other words, to rely solely on our second principle while abandoning the first is doomed to failure. For we are still left with a rule of chance from which to make a prediction, and if we doubt the first principle we cannot do it. If, for example, we find that Mr. Smith belongs to a class of people who, though trade-unionists, have declared their intention of voting Conservative, and if we estimate the chance of such people voting Labour as about 1 in 10, this would leave us, on this view, with no better evidence as to what Mr. Smith was going to do than we had originally. The application of rules of chance to individual cases would be ruled out in the one case as in the other.

Those who appear to accept this conclusion may attempt to mitigate its severity by saying that it holds only when we are speaking of individual cases, and that rules of chance do supply evidence for statements about large sets of things. To say this, however, is surely a mistake. From the rule $C(\alpha, \beta) = p$, we can deduce various propositions about the *chances* that any large set of α things will contain such and such a proportion of β things. In particular we can deduce that the chance that any large set of α things contains a proportion of β things approximating to p, is high. And we would commonly say that this rule thereby provides fairly good evidence for the statement that S, a particular large set of α things, contains a proportion of β things approximating to p. But this evidence is clearly of precisely the same type as that provided by any rule stating high chances for a particular case falling under it. There always may be large sets of α things in which the proportion of β's is nowhere near p, and S may be one of them.

The peculiar advantage gained in making statements about large sets of things is that it enables us, through the use of Bernouilli's theorem—the "law of large numbers"—to deal in chances which are high. If we accept the first principle, this is important—as is realised well enough by insurance companies. It is an advantage which we gain by being prepared to deal modestly in approximate proportions of cases instead of daringly applying low chances to individual ones. But the principle of application remains unchanged.

It is admittedly impossible to meet a critic of this principle by attempting to derive it from anything more elementary. We are tempted to support it by pointing out that the chance of any case being an exception becomes less and less as the value of p increases, and using this as evidence in favour of the particular case, A, not being an exception. But this is merely to convert $C(\alpha, \beta) = p$ into $C(\alpha, \sim\beta) = 1-p$ ("$\sim\beta$" standing for "not-β"), and to use the principle as before, with the rule in its new form. And so on with other considerations of this type. If we feel it necessary to justify the principle at all, we have to do so in some other way. The problem of justifying it in fact raises precisely the same issues as the problem of justifying the use of inductive evidence in support of any law or rule of chance. I do not propose to discuss the question how this problem is to be solved, or whether it is a problem at all. But at any rate we can leave the principle in honourable company.

4. The second principle is the one we often have in mind when we speak of accumulating evidence for and against a given singular statement. We base on it the recommendation to examine the particular case, A, so as to discover as many characters as possible which are relevant in deciding whether or not it is β. To speak of this as accumulating evidence is in some respects misleading. What we are doing is to replace one rule of chance by another, the general by the specific, $C(\alpha, \beta) = p$ by $C(\alpha\gamma, \beta) = p$. The purpose of examining the particular case is to select a specific rule which is applicable

to that case. Both the specific and the general rules are to be understood to rest on evidence, and in their turn to provide evidence for or against the statement "A is β" in accordance with our first principle. What we now say is that the more specific rule, where applicable, provides better evidence than the more general. If, in considering the case of Mr. Smith, we can use rules such as "The chance of a trade unionist who is an active member of the Labour Party voting Labour is $\frac{9}{10}$" or "The chance of a trade-unionist who dislikes the Labour candidate voting Labour is $\frac{1}{3}$", we should do this rather than rest content with the general rule "The chance of a trade-unionist voting Labour is $\frac{2}{3}$".

It must be clear that this applies whether the specific rule states a high chance or a low chance, and even when the general rule states a high one. If A is both α and γ, and there are rules $C(\alpha, \beta) = \frac{99}{100}$ and $C(\alpha\gamma, \beta) = \frac{1}{2}$, we must accept the second and refuse to commit ourselves as to whether A is β. It is of course the case that, given the first rule, the second, though true, will be rarely applicable, for it can be shown that there is very little chance of an α being a γ. $C(\alpha, \gamma)$ must, in fact, be less than $\frac{1}{50}$. Hence if we do not know already that A is γ, we can argue legitimately that it is unlikely that it will be. But if we do know that it is γ, this removes the likelihood of being β which it has when we take into account the first rule alone.

Here then we have a procedure by which we are driven to the use of a highly specific rule of chance, which is applicable only on certain rare occasions of which the occasion under consideration is one. Unless we already know that many of the characters of A are irrelevant, it is a procedure which may go on indefinitely. It may, of course, end with the discovery of a law which is applicable to the case, and so take us beyond considerations of chance altogether. So long, in fact, as we assume that there is always some character which is a necessary condition for the occurrence of β, and some set of

characters which together constitute a sufficient condition for its occurrence, it will always be theoretically possible to discover whether they are present in the particular case. If we knew enough about Mr. Smith—including, for example, precisely what his state of mind was going to be in the polling booth—we should be able to formulate some law from which we could deduce whether or not he would vote Labour. But there will be often great difficulty in establishing such a highly specific law, and we may well go on and on specifying our rules of chance without being able to formulate a law in even the most tentative way.

This being so, we can clearly see an objection to the second principle parallel to the objection that was raised against the first. If there may always be conditions present which require us to replace the rule we are working on at any stage by some other rule in which the value of p is quite different, can we say that the first rule, however many characters it takes into account, is of any value at all as evidence of what is going to happen? To this we can give no better answer than before, namely, that the objection is of the same type as that which can be raised against the use of inductive evidence for laws or rules of chance themselves, and hence that the justification of the principle, if it is felt to be required, must stand or fall with the justification of induction.

Even assuming the principle, however, the need to search for more and more specific laws is clearly a grave defect in arguments from chance. It is not only that there may always be further conditions present which may alter the chances. It is also that the more specific the rule, the fewer the cases (other than the one under consideration) to which it can be applied, and hence the greater the difficulty in obtaining evidence sufficient for estimating the chance even in the roughest way. The difficulty is greater than in the parallel case of establishing specific laws, since we are not able to confirm rules of chance by a search for single negative instances, but have to rely on counting proportions in as large

and as varied a number of cases as possible. It will not be easy, for example, to estimate the chances of a Labour vote coming from a trade unionist of conservative upbringing who dislikes the Labour candidate but has expressed himself in favour of the nationalisation of banking, since there will be only a few people belonging to this class.

For these reasons it often becomes advisable to rely on the first principle rather than on the second, and seek to use rules which are both general and state high chances. For, as we have seen, once we know that A has the general character α and that $C(\alpha, \beta)$ is high, we already know by the calculus of chances, if not by common sense, that there is only a slight chance of any α thing (A included) possessing other characters which will radically alter the chances. Thus, once we know that Mr. Smith is a member of the Labor Party and that the chances of members of the Labour Party voting Labour are high, we already know that there is only a slight chance of such a person having his chances of voting Labour reduced by becoming subject to sudden political disillusionment or to a fatal disease which may kill him before the election, or some other such special character. Hence it is only when we are unable to employ general rules stating high chances that the application of the second principle, with all its difficulties, becomes important.

It is in this connection that we must notice a further advantage in drawing modest conclusions about approximate proportions in large sets rather than straightforward ones about individual cases. By drawing such conclusions we do not avoid the need to employ the second principle, or the difficulties which this involves, any more than we avoid the need to employ the first. Any particular set, S, of α things may always contain such proportions of things possessing other characters—γ, δ, and so on—as will require us to replace a rule about the chances of approximate proportions of β's in an α-set by another which makes reference to these other characters. Thus, given that the chance of a manual worker voting Labour is $\frac{2}{3}$,

we may have as our first rule "The chance of any set of 1000 manual workers containing a proportion of about $\frac{2}{3}$ Labour-voting members, is high". If, however, a particular set, S, happens to contain a large proportion of active Labour Party members, it will come under a new rule in which this chance will be reduced, the high chances moving in favour of some higher proportion. It is clear that our second principle is operating here, only in a more complex form.

And yet we do gain a considerable advantage, in considering approximate proportions in large sets, from being able to rely on dealing in general rules giving chances which are high. For this ensures that there is only a small chance of the chances of the approximate proportion holding for any set being seriously altered by its having some special constitution. In any sufficiently large collection of manual workers, there is not much chance that there will be such an undue proportion either of active Labour Party members or of people who dislike nationalisation or of whatever it may be, as will greatly alter the chances of such and such an approximate proportion of them voting Labour. This can be put in a common sense way by pointing out that while the individual members of a set may have a variety of characters which would bring them under various specific rules of higher and lower chances, it becomes more and more likely, as the size of the set increases, that these deviations will cancel out. Thus, by dealing in sets, we short-circuit to some extent the multiplicity of relevant conditions which would otherwise force us into using ever more specific rules.

5. At this point, however, we must consider one further way in which we might try to avoid the difficulty about the formulation of ever more specific rules in which the second principle seems to involve us. This would be by drawing upon general rules of chance and combining them in such a way as to give a specific rule appropriate to the case we are considering. On this procedure, if we wished to know the value of $C(\alpha\gamma, \beta)$, we would consider the values of $C(\alpha, \beta)$ and $C(\gamma, \beta)$,

and put them together to give some composite result. If, for example, with Mr. Smith in mind, we wished to estimate the chance of a trade-unionist who dislikes the Labour candidate voting Labour, we would consider the general chance of a trade-unionist doing so, and then correct this in the light of the chance of a person doing so who dislikes the Labour candidate.

If we could argue in this way, it would undoubtedly be of very great assistance. It would mean that, instead of having to establish each rule of chance separately, we could develop systems of such rules, similar in kind to the systems of theoretical laws which are used in mechanics. And given a few general rules, we would be able to derive a great variety of specific rules for application to particular cases.

Now there is no doubt we have a tendency to feel that we can argue in this way. We feel that two favourable chances (*i.e.* chances greater than $\frac{1}{2}$) should add up to a still more favourable one, and likewise that two unfavourable chances should add up to a still more unfavourable one. Suppose, to take an example of John Stuart Mill's,[6] that where a person who has been near the scene of a crime has attempted to disappear there is a more-than-even chance that he is the culprit, and suppose likewise that there is a similar more-than-even chance where such a person has been found to have blood-stains on his clothes. In such a case we feel inclined to argue that the chance of a person being the culprit where he has both tried to disappear *and* has blood-stains on his clothes is higher than either of the two separate chances.

Again, we feel that where one chance is favourable and the other unfavourable, we should subtract the latter in some way from the former, so as to give a chance lying somewhere between the two. In the case, for example, of there being a strong chance of a trade-unionist voting Labour, and only a small chance of a person with a dislike of the Labour candidate voting Labour, we feel inclined to assume that the resultant

[6] J. S. Mill, *System of Logic,* Book 3, Ch. 23, Sect. 6.

chance would be lower than the former and higher than the latter. Just as in the first type of case we seem to have an account of the corroborating of one piece of evidence by another, so in the second we seem to have an account of the weighing of one piece of evidence against another.

Unfortunately, however, this feeling of ours appears to rest on a confusion. There is no way of deriving specific rules of chance from general ones in the manner which has been suggested. The following two considerations should make this clear.

In the first place, it is difficult to see what principle there could be for compounding chances which would satisfy our requirements. Consider, for example, the principle proposed by John Stuart Mill. He argues as follows:[7] Suppose that $C(\alpha, \beta) = \frac{2}{3}$ and $C(\gamma, \beta) = \frac{3}{4}$. Then, out of every 12 things which are both α and γ, the chance will be that 8 will be β on account of α alone, and then, among the remaining 4, the chance will be that 3 will be β, on account of their being γ as well as α. Hence the resultant chance, $C(\alpha\gamma, \beta)$, will be $\frac{11}{12}$. The general formula of which this is an illustration would be $C(\alpha\gamma, \beta) = p + q(1 - p)$, where $C(\alpha, \beta) = p$ and $C(\gamma, \beta) = q$.

This looks plausible, and satisfies our requirement that where there are two favourable chances, the resultant chance is higher than both. The trouble is, however, that it would make the resultant chance higher in all circumstances, even when one or both of the chances were unfavourable. And this would be against our requirement that the resultant of two unfavourable chances should be lower than either, and the resultant of one favourable and one unfavourable one should be somewhere between the two.

For the case of two unfavourable chances, this might be put right by proposing a complementary principle. This in fact is what Mill, by implication, proceeds to do in the second

[7] *loc. cit.* Mill, admittedly, had doubts about this argument in the 7th edition, but subsequently reinstated it.

part of his argument. He argues that where the constituent chances are $\frac{1}{3}$ and $\frac{1}{4}$ the resultant would be $\frac{1}{1\,2}$, the general formula in this case being $C(\alpha\gamma, \beta) = pq$. If this and the preceding formula were used each in its respective sphere, the ground would to that extent be covered, the only alarming feature being that there could never be a resultant chance falling between $\frac{1}{4}$ and $\frac{3}{4}$.

The case of the weighing of the unfavourable against the favourable chance would, however, still remain unaccounted for. We might, of course, meet this case by proposing yet another principle. We might suggest, for example, that, taking the point of even chances as a starting point, we add and subtract the amounts by which the respective chances are in excess of, or fall short of, this. The formula here would be $C(\alpha\gamma, \beta) = \frac{1}{2} + (p - \frac{1}{2}) + (q - \frac{1}{2})$. This again would be plausible, but when this stage had been reached, the multiplicity of the principles of combination and the discontinuity between them would begin to make the whole procedure suspect.

To this we must add the second and more fundamental consideration. This is that any such principle that we might formulate would lack logical necessity. We can perfectly well conceive a combination of α and γ giving rise to a chance which bears no relation to the constituent ones. Hence, when Mill claims that his principle is a deduction from the calculus of chances, he is simply making a mistake. There is, in the calculus of chances, a theorem whereby we can, with precautions, derive $C(\alpha, \beta\gamma)$ from $C(\alpha, \beta)$ and $C(\alpha, \gamma)$, but no corresponding theorem for deriving $C(\alpha\gamma, \beta)$.

Furthermore, it is not difficult to point to cases where the sort of principle which has been suggested does not hold. Suppose, for example, that the chances are in favour of a trade-unionist voting Labour, and against a person of conservative upbringing doing so. Might we not nevertheless find that the chance of a trade-unionist of conservative upbringing voting Labour was even greater than that of trade-unionists in general? If we found this, we would explain it in a general

way by saying there is a strong chance that people of conservative upbringing who become trade-unionists will react against their former political views. Again, though there is a good chance that a suspect who possesses a gun of the right calibre committed the murder, and an equally good chance that one who quarrelled publicly with the victim shortly beforehand did so, the combined chance would seem to be less than either, since there is a strong chance that people who carry guns with intent to murder will avoid public quarrelling with their victims.[5] Such cases are by no means rare, and even if they were, there would be no way of knowing beforehand when they were likely to occur. Hence there is in fact no alternative but to consider in each case what the chances arising from the combined presence of α and γ will be.

We must therefore dismiss the idea that we can derive specific rules of chance from general ones, and that we can thereby build up systems of rules of chance as we can sometimes build up systems of theoretical laws. Our tendency to think that we can argue in this way must be explained as part of the very general confusion between estimating evidence in support of our conclusions, and establishing rules of chance. No one doubts that we can accumulate evidence in favour of a proposition, or that we can weigh one piece of evidence against another. The mistake comes when this process is interpreted as if it were a matter of calculating chances in accordance with formal rules.

We conclude then that the difficulty in the application of our second principle remains. Since each rule of chance has to be established separately, it follows that the more specific they become, the more tenuous will be the evidence we can produce in their support. Hence the recommendation stands that in arguing from chances we should, where possible, find general rules stating high chances, and apply the first principle.

[5] *Cf.* Kneale, *op. cit.*, pp. 128-9, for a statement of this point, and a further illustration.

THE MOVING 'NOW'

By J. J. C. Smart

Why do we feel like talking of 'Now' as something which moves inexorably on, in a way in which we do not feel like talking of 'the moving Here'? Of course there is a sense in which we might be tempted to say that 'here' is the name of something that moves. If I am in Edinburgh 'Here' is in Edinburgh; I catch the South train and 'Here' moves to London. If the train moves at 60 m.p.h. we could even say that 'Here' moves at 60 m.p.h. It would be a peculiar thing to say, but it would not lead us into immediate trouble. In this case 'Here' moves at 60 m.p.h.; later perhaps it stands still. We also feel like saying that 'Now' moves. But unlike 'Here', 'Now' cannot stand still. It seems a queer sort of motion that *logic* forces on you; it is a peculiar thing to say that one moves if there is no logical possibility of one's not moving. The logical incoherence of our thoughts comes out when we ask ourselves how fast 'Now' moves. We could answer the analogous question with 'Here' but 'Now' leaves us baffled.

Suppose I stay at my desk all morning. Then 'Here' has not moved. At different times the word 'here' has pointed to the same place. On the other hand, at different times 'now' has of course pointed to different times. To say this is to say no more than the tautology that different times are different times. So if we say that 'Now' cannot help moving whereas 'Here' sometimes stays still, we are misled by a false analogy between two similar looking but very different propositions. 'At different times "here" points to different places' is a contingent proposition. It is true or false according as I do or do not move during the stretch of time in question. On the other

hand the proposition 'at different times "now" points to different times' is not contingent but is an elucidation of the meaning of 'now'. At first sight a better analogy with 'at different times "here" points to different places' would be this: 'at different *places* "now" points to different times'. Let us examine this possibility.

Consider a man walking along a road. Suppose that he is at milestone 3 at 10 o'clock, at milestone 6 at 11 o'clock, and at milestone 9 at 12 o'clock. If he says 'now' at milestone 3 he refers to 10 o'clock, if he says it at milestone 6 he refers to 11 o'clock, and if he says it at milestone 9 he refers to 12 o'clock. So it seems right to say that in this case at different places 'now' points to different times. There is, however, something queer about this. For in the case of 'at different times "here" points to different places' we can think of a case (that of a moving man) in which the proposition could plausibly be said to be true and we can think of another case (that of a stationary man) in which the proposition could plausibly be said to be false. But what sort of case could we think of to illustrate the falsity of 'at different places "now" points to different times'? A man who said 'now' at the same time but at different places would have to be in two places at once, and it is contrary to the grammar of 'man' or any other thing word to say that a man or a thing could be in two places at once. The asymmetry between 'at different times "here" points to different places' and 'at different places "now" points to different times' is closely connected with the logic of thing words (in traditional terms, the logic of Substance). We say that a thing endures or changes. Though, in an everyday sense, a thing is a solid, nevertheless 'thing' has not the logic of 'solid' in 'solid geometry' (3-dimensional geometry). For in 3-dimensional geometry time does not enter at all. But neither does 'thing' have the logic of 'solid' in the 4-dimensional geometry of space-time, for in this 4-dimensional representation we would not talk of a thing changing but only of its 3-dimensional (spatial) cross section for $t = t_1$ being different from

what it is for $t = t_2$. In the 4-dimensional representation time does enter in, but not the terminology of change or alteration. If we say that 'solid' in 3-dimensional geometry has a 3-dimensional logic, and that 'solid' in 4-dimensional geometry has a 4-dimensional logic, then 'thing' or 'substance' has neither a 3-dimensional nor a 4-dimensional logic, but a sort of monstrous hybrid between the two. This introduces an asymmetry between the logic of 'time' and that of 'place' which explains the asymmetry between 'at different times "here" points to different places' and 'at different places "now" points to different times'.

There are clearly two different senses of the word 'space'. In one sense 'space' is something that endures. It has some of the logical properties of 'thing' or 'substance'. (We might say that a thing was at the same place at different times, or that a place continued to be unoccupied.) In another use of the word 'space' (and this is the mathematical one) space is a timeless entity. If we talked, say, of a pyramid turning into a cube we should be moving outside the bounds of solid geometry. It is in this sense that we use the word 'space' when we talk of the space-time of the Minkowski representation as 'a 4-dimensional space'. (Clearly within the Minkowski representation we must not talk of a four-dimensional entity as *changing*. We should need a fifth dimension for its change to be *in*.) I once elsewhere drew attention to the danger of 'spatialising time'.[1] That is, I was drawing attention to the danger of treating time as though it were space in the *first* sense of this word. (As something which itself endures.) Clearly there is no objection to spatialising time in the sense of treating time (or space-time) as space in the *second*, or mathematical sense of the word. Spatialising time, in *this* sense, is a perfectly proper, indeed a laudable thing to do. In the 4-dimensional representation we do not talk of a thing

[1] *Mind*, Vol. 58, 1949, pp. 493-4. So also does G. E. Hughes, 'On having the past over again' (Inaugural Lecture, Victoria University College, New Zealand, 1951).

changing but of a 4-dimensional solid having differing 3-dimensional cross-sections. We talk about the same facts but the form of representation is different.

We can see, then, that the differences between the (almost) sensible statement 'at different times "here" points to different places' and the definitely queer statement 'at different places "now" points to different times' indicate something of importance about the concepts of 'thing' and of 'place'. But the statement that gives rise to the feeling of 'the moving Now' is that 'at different times "now" points to different times'. This, we saw, is tautologous. It cannot really be compared to that which states the existence of a moving 'Here', namely the statement that 'at different times "here" points to different places'. The latter is almost a sensible proposition—we could give it a meaning so that if it was said by a man in a moving railway train it would be true and if said by a man at home in bed it would be false.

REVIEW ARTICLE

PSYCHOLOGICAL MORALISM

MAN, MORALS AND SOCIETY, A Psycho-Analytical Study. By J. C. Flugel. Duckworth, London, 1945. Pp. 328.

This work resembles other productions of the time of its publication, and especially other productions by psychologists, in professing to have a practical purpose, that of finding a way out of the "tragic tangle" in which society has become involved. The psychologist, recognising that "both the failures of the past and the problems of the present and the future are to a large extent psychological in nature", may feel a certain shame at the failures but will also feel challenged by the problems to review the relevant facts and theories "with some hope that such a scrutiny of available data will reveal him, both to himself and to his fellow-men, as one who is not altogether doomed to gape idly and uselessly at the scene of human tragedy, but rather as one who can at least here and there make a promising suggestion or lend a helping hand in the work of salvage and reconstruction" (p. 9).

Flugel follows up this initial statement of the psychologist's responsibility by saying that it "is pretty generally agreed that the problem of rebuilding our tottering society upon a sounder basis is to some extent a moral problem, in the sense that its solution depends upon an appeal to the moral impulses of man", and that, while some knowledge of the origin and nature of these impulses is required if the appeal is to be successful, recent psychology has in fact gained such knowledge of these impulses as it may be possible now to organise and fruitfully apply. With this may be compared the remark in the Preface (p. 5) that "it appears to be pretty generally agreed that the failure of our civilization to solve so many of its greatest problems, and above all its involvement in two world wars within a quarter of a century, makes it more than ever necessary that we should think seriously about fundamental

moral problems"—this book having the particular task (p. 6) of considering "the possible bearings of the recent psychology of moral motives upon the ethical problems of an admittedly distracted world".

Flugel thinks it possible to make this response to the "urgent demand for a revision of ethical thought" without any "general treatment of the nature and problems of ethics" (and likewise of psychology and psychopathology). But, leaving that point aside for the moment, we may note particularly his conception of a problem and of its solution—viz., not by finding that something is the case but by determining that something is to be done. This is the very opposite of the attitude of *serious thinking* on moral or other problems, the attitude of disinterested study which not merely stands aloof from practical urgencies but subjects them, and the conceptions in terms of which they are expressed, to rigorous examination. The only thing that could properly be called a failure in our civilisation would be a failure in criticism; and, of course, the serious or critical thinker knows that, whatever ups and downs criticism may have, it has in any case to maintain itself by struggle in an uncritical and "practical" environment, and that there is no question of a "solution" in which that struggle will disappear. But the raising or lowering of critical standards is not apparent to the vulgar, and the doctrine of a conspicuous breakdown or a conspicuous uplifting of civilisation is itself a vulgar view.

But while, without a "general treatment" of ethics, involving a criticism of ethical conceptions, no important contribution to ethical theory can be made, it is only fair to recognise that practicalist confusions have been very much encouraged by the mass of moral theorists, that they have treated ethics as a special kind of science whose propositions have a special kind of truth, practical truth, or a peculiar copula, the practical or preceptual copula. Thus, although Flugel uses when it suits him all the odds and ends of conventional ethics (desirability, rationality, altruism and so forth),

although he never could succeed in substituting psychology for ethics, his efforts in that direction are at least attempts to substitute something which can be inquired into for something which, as conventionally conceived, simply blocks inquiry. To that extent, however he may from time to time smuggle in his own precepts, he casts light on the conditions under which *ethics as a science* (i.e., as a positive science, a subject of empirical investigation) can emerge.

Flugel's argument in the first chapter ("Psychology and Morals") is of particular importance here. He is seeking to justify the contention that psychology can contribute to our understanding of "the field of values" by investigating "the motivations underlying values", and, to that end, to discount the objection that psychologists, in professing to make such contributions, are going beyond their province. "Psychology, we are reminded, is a positive, not a normative, discipline, that is, its business is to describe, classify, and (if it can) explain the facts of mental life, just as physics and chemistry deal with the facts of the material universe. Like these latter sciences, it has no concern with values as such; it must take the facts as it finds them and must not presume to pass judgment on their desirability or undesirability" (p. 11). Flugel remarks that this general position is one with which few if any psychologists would wish to quarrel—and then goes on to a series of considerations which, if they had any force at all, would require its abandonment. But the primary point is this, that if there were the supposed distinction between facts and values, then not only would investigation of facts cast no light on values but there could be no investigation of values at all, and any suggested connection between a fact and a value would be entirely arbitrary—the value would be somehow "annexed"[1] to the fact but would not *belong* to it and might as

[1] In most cases *phrases* in quotation marks are Flugel's even when no page reference is given. But quotation marks round single expressions are generally intended to indicate some extended or uncommon usage the nature of which I think should be clear from the context. The word "annex", of course, is not used by Flugel.

well be annexed to any other fact. More exactly, if "values" have any content, any positive character, they must be studied by the same methods and in the same situations as other things; they must be found, like minds, in what Flugel calls "the material universe" (as if there could be several universes), i.e., as objective occurrences. It is the dualistic outlook, with the absurd attempt to attach something of one kind of reality to something of another, that has obstructed ethical *study*, while at the same time it permits everyone to "have his fancy".

This is why Flugel does not throw over "values" altogether; he wants to retain a position in which anyone is expert, to be able to insinuate his values at any stage in his investigation of the facts. Thus the considerations by which he hopes to show the relevance of psychology to moral theory have to be expressed in such a way as to leave an opening for "annexings". His points are (1) that values "happen to be facts of *mental* life", (2) that a distinction must be made between pure and *applied* science, (3) that the distinction between means and ends "is nearly always relative", and (4) that, in the substitution of the psychological for the moral point of view even in the sphere of "intrinsic values", we are replacing moral judgment, "primarily an orectic process", by scientific judgment, "primarily a cognitive process". In these and other ways the psychological is "tending to replace the moral point of view, and there is little doubt that, in so far as the new approach proves effective, the process will continue" (p. 16). But either this is asking science to do something it cannot do or science *does* provide us with "norms". Flugel straddles the issue by saying that science, while it "may never give us ultimate values", may still, as it advances, "be of help in ever higher levels of the hierarchy of values"; but since he immediately goes on (beginning of ch. II) to say that moral action "is action in accordance with values" and that fundamentally "these values are determined by our biological nature and our innate psychological equipment", it is clear that his reservations are merely such as to permit the scientist,

with *his* equipment, to give upon occasion directives as well as findings.

On the first point we are faced with the common -ing and -ed confusion; it is not made clear whether the question is of *what values* or of *what is valued*, and it is quite possible to take the view that, while the former is mental, the latter is not and would not have its character in any way illuminated by psychological investigation. Of course, if it were a question of a definite quality *good*, it might well be held that both what observes goodness and what is good are mental processes, though it still would not follow that the study of the former would cast any light on the latter. But it is clear enough, from the phrases quoted above, that Flugel does not make these distinctions, that for him values exist in the processes which "annex" them to various things (cf. the remark, in the note to p. 111, that, in the last resort, "it can be maintained that all so-called objective values are ultimately subjective in origin, inasmuch as things in the outer world are good or bad only in virtue of our attitude to them")—clear, too, that no amount of investigation of the positive characters of either mental processes or the things they are cognisant of can show what such "annexings" even *mean*. On the other hand, if we do take the positive view, if we consider, in particular, that goods (good things) are a species of mental activities, then psychological science can be of assistance to ethical science (a) by its formulation of general laws of mental process, (b) by bringing out characters of the other mental processes among which good activities exist and with which they interact; but psychological study which was not direct study of good activities could not itself be a substitute for such direct study —it could help ethics only if ethics were an independent study of certain facts.

So, if there were definite things describable as "values", psychology could conceivably indicate conditions under which they come about, but this would imply no distinction between the kind of science that studied them and the kind of science

that studied these conditions, and would, in particular, do nothing to justify a distinction between pure and applied science. According to Flugel (p. 12), whereas "pure science is concerned with things as they are, its only aim being knowledge for its own sake, applied science seeks to use this knowledge for the attainment of certain ends, ends which are assumed to be desirable and which therefore imply certain values (over and above the mere values of truth or knowledge). Thus medicine or engineering imply values in a way that physiology or physics do not; they imply that it is desirable to achieve and maintain a person's health or to construct and keep in order a machine". This, of course, is not the case. True propositions of medical science imply further true propositions of medical science, but they do not imply anything in the way of a policy, nor is any such thing inherent in them. If we desire A, and if we *know* that it comes about under conditions B, and if we are able to bring about conditions B, then we are in a position to satisfy our desire; but this does not entitle us to speak of "applied knowledge" or "applied science" as if it were a special *kind* of knowledge or science. Granting, then, that psychological knowledge may be utilised "in the fields of medicine, education, and industry", what is so utilised is "pure" psychology, not "applied" psychology. And if such utilisation is held to require that certain "ends" should be "assumed to be desirable", then, if this is not a psychological assumption (the assumption that certain propositions in the field of psychology are true), either it is an assumption in some other, equally scientific field—but one taken to be continuous with the psychological field—or it is a mere confusion and in no way elucidates the conception of "application".

In fact, continuity is the vital point. The ambiguity of "desirability" (a much-debated matter on which, it need hardly be said, Flugel does not touch) is one of the devices enabling the dualist to jump the chasm between his antithetical realities —or rather to appear to do so without inconsistency. But, as before, there can be no connection between a thing and its

supposed "value" unless this is as much one of its characters, part of its "constitution", as any of its other characters. Of course, the thing has various *relations*, but these will also be studied as matters of fact and within continuous situations, and there is still nothing here to support the sort of distinction suggested. The consideration of the relation of desiring or having ends, however, points to another form of discontinuity besides that which is masked by the *annexing* of "values". This is the discontinuity between the agent and the act, between that which applies and that which is applied, between that which annexes and that which is annexed or that *to* which something is annexed. Unless the mind or person is of the same order as the phenomena in which it is taken to be involved and is subject to the same sort of (indeed, to the same) investigations, there can be no way of saying that any acts or processes are *its*, any more than of saying that the "value" of anything is *its* or can be really *assigned* to it. And, while psycho-analysis has done much to support a pluralistic and empirical view of mind, a residual dualism still appears in its individualism—in the conception of unique agency which, not merely in Flugel's work but in the great mass of psychological and sociological literature, appears in conjunction with voluntarism, i.e., in the substitution of such questions as "What are we to do?" ("How shall we *apply* our knowledge?" etc.) for "What are minds and how do they proceed?". The dualistic and discontinuous "agent" is perhaps the greatest barrier to the advance of both psychological and ethical science, and "desirability" is a good example of the confused conceptions by which it is bolstered up.

It will follow from what has been said that nothing is in itself (or in its own nature) an end and nothing is in itself a means. But Flugel's statement that the distinction between means and ends is "nearly always" relative is not (as indeed the reservation itself would show) based on logical considerations. It follows immediately upon the statement that it is the business of ethics to decide what the "higher values" are

(so that applied psychology, like other applied sciences, "is concerned with 'means' rather than with 'ends'"), and is the beginning of an attempt to whittle away that concession to ethics. Thus (p. 13): "At best there can only be a few unquestionably intrinsic values at the top of the hierarchy, such as Truth, Goodness, Beauty; or, if we press the matter further, there should strictly speaking be one only, a *summum bonum* or supreme value, to which all the rest are means—and, as we know, moral philosophers are not yet in agreement as to what this supreme value is." There is no real obstacle, then, to psychology's supplying its own supreme values, and indeed the whole of Flugel's book is propaganda for a psychologists' morality, a morality of a weakly humanitarian type but one just as entitled as any other morality to annex its values to facts to which they are actually extrinsic.

Flugel's fourth point, however, is an attempt to have things both ways, to make his peculiar morality prior to morality as such. "The substitution of the psychological for the moral point of view in any matter implies also a change in mental attitude—a change from a relatively emotional attitude to a relatively intellectual one. Scientific judgment is primarily a cognitive process, moral judgment—in this respect like judgment in matters of religion and aesthetics—primarily an orectic process. But with regard to difficult and delicate problems, cognition is often more effective than orexis" (p. 14). Flugel goes on to say that we do not pass moral judgments on inanimate things and scarcely at all nowadays on animals, and adds that "this restriction of moral judgment and the substitution of judgments in terms of psychological insight is rapidly increasing, even in our dealings with fellow human beings, and for much the same reason as elsewhere, namely that it is so often more effective". Among recent examples of this is the attempt in education "to substitute understanding for censure; it is recognised that *it is better* to find out why a pupil is lazy or stupid than it is to blame or punish him" (p. 15). The phrase I have italicised here (suggesting the

question whether this is a scientific or a moral judgment) is not a mere slip; it exposes what is inadroitly covered up in the expression "effective", where the question is clearly not just of having effects but of having "desirable" effects. But, quite apart from this smuggling in of what was supposed to be set aside, the major contrast proposed by Flugel will not stand up. If there is such a thing as "moral judgment" at all, then it is judgment that something is so, and it is just as "cognitive" as any other judgment. But to say that something is cognised, that some proposition is regarded as true, is not to say what are the characters of *that which cognises* and is nothing against its having such characters and relations as are conveyed by "feeling, striving, and wishing". Thus no distinction between types of judgment has been brought out. The underlying point, of course, is that an "orectic" judgment is not a judgment that something is the case but a judgment that something *is to be* the case—and if we admitted such judgments, we certainly could not regard them as helping inquiry; we do not find out what X is by laying down what X is to be. But, in fact, "is to be" judgments, confused as they stand, are always elliptical, and it is the task of criticism to show what are the unstated purposes to be served or what are the forces concerned to *make* an X be Y. Flugel, however, cannot clear the matter up because, as we have seen, his "psychological point of view" is just another moral one, an attempt to annex a particular set of "values" to the facts, a treatment of *science* as saying what "is to be".

The conception of a peculiarly "cognitive" judgment is akin to that of "rationality" in thought and behaviour. Rational cognition is that which knows the reasons for itself; rational action is that in which we at once know what we are doing and why we are doing it. These are to such an extent underlying presuppositions of Flugel's work, he is so far from imagining that they can be questioned, that he nowhere formulates them, let alone discussing them, but it is only in such terms that the distinction between the kinds of judgment or

between reasonable and unreasonable procedures is intelligible. In fact, however, there is no such thing as rationality in the required sense. Whatever reasons are found for anything, the point of departure of the reasoning is always something that is simply found (without reasons); whatever we know about processes either in or out of ourselves, we never know "all about" them. The confused doctrine of "ideas", of entities which are just what they are known as because "what they are known as" is precisely their nature or meaning as ideas—confused because the "they" here could have no content or, as it might alternatively be put, because the attempt to assign one would involve an infinite process—is paralleled by the confused doctrine of "conscious action", action which, in carrying it out, we know all about because it is just our awareness of it that *makes* it conscious action. But, apart from formal objections (decisive though they are), the important point is that such self-wrapped entities could not be connected with anything else, could in particular have no transactions with other things, no history intertwined with other histories. The importance of the Freudian theory of the "unconscious" lay not just in its dispelling of formal confusions but in its indication of a concrete, continuing thing with its own characters, no matter how much or how little it might know or be known at any given time; and one condition of the working out of this theory, of the study of the transactions between the continuing mind and its surroundings, was the recognition of its complexity, of its *internal* transactions (including conflict) continuous with its external transactions. The rejection of the conception of the "unitary person" went with the rejection of the "conscious self".

Unfortunately the Freudians, including Freud himself, were unable to maintain this position or to work out the consequences of the initial revolution. The question became one not of the rejection of consciousness or self-awareness (the assertion of the distinction between a thing's own characters

and its relations) but of the *restriction* of consciousness to a particular mental region, and the doctrine of "ego, id and superego" which finally emerged, and which now dominates the work of the Freudian school, was largely a reinstatement of individualistic or atomistic thinking. Almost two-thirds of Flugel's book (chs. IV to XV) is devoted to consideration of "these three main parts or aspects" of the mind, with special emphasis on the superego as "the source of our moral control", i.e., as exercising a mandatory, and especially prohibitory, function. The consideration is not, of course, critical; it is mainly a setting out, in orthodox Freudian fashion, of types of mental conflict or difficulty, leading up to the major problems which "the human race must solve or perish". There is, in particular, no criticism of functional definition, no suggestion of the possibility that the very same thing could function in the various ways taken to be characteristic of the main mental agencies. Thus, even when it is admitted (p. 198) that there can be "righteous indignation" against an "authority", this is taken to indicate "a split in the superego" and not to undermine the whole conception of *the* superego. The argument throughout is dominated by the conception of the individual agent, exemplified in the personification by which the superego is said to "oppress" the ego or the ego to "defend itself" against the superego, and in the acts of the individual, acts of "introjection" and "projection" in particular, by which the superego, the "moral authority", is built up.

Flugel does not appear to find any difficulty in recognising such processes; jumping across barriers, throwing a content out or in—these are just things that the individual can do. But in fact it is only in terms of continuity among lives, of participation in social activities, that they are even conceivable. Just how important is the part played by *authority* in moral life is another question. The main point is that a person's development of moral characters and his recognition of them depend alike on his coming to participate in con-

tinuing "ways of life", forms of activity, which do not depend on him either for their existence or for their character. And just as the egoistic treatment of these questions, the postulation of a separate agent with his distinct acts of acceptance and rejection, is unintelligible, so is the treatment of *good,* in particular, as something which "I" select or pursue. It would, in the first instance, be a low moral view which made good *subject to the choice* of something else which was presumably not good; and the alternative is to treat good not as something which this or that person does or pursues but as something which itself operates in characteristic ways, something which may indeed operate within a person but which does so as being of some positive quality or content and not as an empty "agent". If such a positive view is not taken, then the subject "ethics" simply disappears and there is nothing for psychology to illuminate. But if it is a question of types of activity with their distinctive qualities, then we can see not merely that such activities do not stop at the boundaries of a person, that they pass continuously between persons, but that the understanding of them casts light *on psychology,* that the operations of a mind are strikingly illuminated by a knowledge of the "ways of life" among which it exists and develops. It is perhaps just because ethics casts more light on psychology than psychology does on ethics that psychologists are led to *fabricate* an ethics or to make their psychology *stand for* ethics. But at least there will be no coherent theory on the supposition that either moral forces or ethical conceptions are manufactured by "the individual"—or indeed, as seems to be the position of the Freudian school, by the *infant.* The question is of participation, of the ways in which things and persons *belong together* in concrete forms of activity or ways of living.

Religion and punishment (each of which bulks fairly large in Flugel's discussion) are examples of social phenomena which are not accounted for, or even illuminated, by the attribution to the infant mind of processes of "introjection", "projection" and so forth, but which, as features of the life in

which that mind is embedded, help to determine the character of its interchanges with its surroundings or exhibit *forms* of such interchange. It is in terms of the social (not the individualist) theory of religion that we can understand its history and its impact on particular minds—in terms, that is, of the distinct departments of social life and of the conditions of keeping them going, of the rites which were originally *part* of the type of activity in question but became separated off as *symbols* of all that they "belonged together" with, of the "spiritual agencies" which became increasingly personified as beings *presiding over* the various departments though their objective content was just these "provinces", these continuing forms of social activity, themselves. If "the needs underlying religion" were of "infantile origin" (p. 271), if the "projection of the superego" (ch. XIII, esp. pp. 186 ff.) were relevant to the formation of religious systems, we should expect monotheism to be religion's primitive form. But in fact polytheism is prior to monotheism, just as participation (or social function) is prior to individualism.

It is in these terms too—it is in that precise connection—that we can understand punishment or "sanctions". The question is of resistance to "encroachment" or of the rectification of boundaries, of the bringing together again of things which "belong together" when some breach of their connection has been made. How far sanctions should go, how far the repairing of injuries to the continuity of social processes requires the imposition of penalties on the offender, is a special question. The offender himself may make good the damage, or official custodians of continuity may do so by "making an example" of the offender. The point is, in any case, that the offence itself has involved a loss of participation, a breaking up of established connections, in social life and not merely on the part of a particular offender; and it is natural enough that the situation of the invasion of rights should be met by a certain disfranchisement or curtailment of rights, a loss, greater or less, temporary or permanent, of the privileges of participa-

tion. But the set of social phenomena which can be rendered coherent by the notion of disfranchisement or loss of participation, remain unilluminated if the fundamental feature of punishment is taken to be the infliction of *pain*.

The notion of "belonging together" is not, of course, a universal solvent. It is precisely characteristic of primitive thought to *identify* things which are merely associated in social activity and thus (as in magic and fetishism) to treat each thing as embodying the whole power of its province—though this will at least stand comparison with atomistic doctrines of individual entities with their *separate* powers. Again, it is impossible to maintain boundaries and avoid encroachment, if only because things and persons belong to different departments of social life or have many "social functions"; and while primitive thought dimly realises this (as in stories of wars among the gods) and tries to counter it (as in the conception of Moira, or proper apportionment, which "governs even the gods"), it cannot really grapple with what this implies—the inevitability of social change, the impossibility of the indefinite continuance of the forms of social (tribal) activity in existence at any time. But this is only to indicate the unsoundness of a totalistic view, not the soundness of an atomistic view. Participation is fundamental alike to a scientific ethics and to the confused ethical conceptions of the *fitting* or proper, the obligatory and the desirable—that which has such characters as make it a proper thing to desire, as enable it to fit into a certain scheme.

The point is that communication is limited, that there will always be forces opposed to it, that there will always be social conflict—conflict, in particular, between an objective and critical attitude to things and a subjective and uncritical attitude. This is something which Flugel with his humanitarian and progressivist outlook cannot admit, and that is why he cannot really get to grips with ethics, why he has to try to turn it into something else. In his summarising chapter (XVI) on "The Psychology of Moral Progress" he takes as "guiding

notions concerning the main lines of moral progress and development" (1) from egocentricity to sociality [but it is only to *altruism*], (2) from unconscious to conscious, (3) from autism to realism [i.e., in both cases, to "knowing what we are doing"], (4) from moral inhibition to spontaneous 'goodness', (5) from aggression to tolerance and love, (6) from fear to security, (7) from heteronomy to autonomy, (8) from orectic (moral) judgment to cognitive (psychological) judgment. These are treated in an essentially preceptual and thus subjectivist manner. Autonomy, e.g., is conceived simply as independence of judgment and not as the objectivity, the independent working, the irreducibility to anything else (as Kant half saw), of the subject-matter of ethics itself—the objectivity of goods. Aggression, again, is dealt with by precepts and pious hopes; on the one hand, if it is a need like hunger, the most we could do "would be to discourage [how?] the aggressive equivalent of gluttony and to find [how?] the least harmful and destructive channel for the remaining irreducible aggression"; on the other hand, if it "is purely a reaction to frustration, we can, in theory at least, hope to diminish it by reducing [how?] the frequency and intensity of frustration" (p. 249). The appeal is never to the laws of social science; the assumption is always that whatever the right-minded resolutely decide to do has at least a good chance of coming about.

Flugel himself appears to support the second view of the nature of aggression. At any rate, he ends the last chapter ("The Problem of War and Peace") with a clarion call for the turning of aggression into the battle of man against nature and not against his fellow-man. "It has been chiefly in war that they [men] have sought and found the sense of high adventure; and brotherhood in arms has up to now been the supreme form of co-operation. It is only in recent times that they have been able to see at all clearly the possibilities and implications of the goal of Progress; and even now they have hardly begun to realize that Progress can be an ideal embracing

and inspiring all mankind—an ideal that still calls upon men to be brothers-in-arms, not against their fellows, but against the forces of nature which, in so far as they threaten, restrict, and embitter human life, are the enemies of all. If we wish to be dramatic (and it is perhaps well that we should be so, if we would compete against the lure of war), we can say that the stage is set for the epic struggle of Man versus the Universe—a spectacle surely no less breath-taking in its audacity and splendour than the most famous exploits of purely interhuman warfare" (p. 321). Flugel wonders whether those who have shown heroism in war will also have the courage and the insight to enter on the struggle "in which all mankind can be allied", and, allowing that what is primitive and sinister in human nature might make us doubt whether "such a thing is possible", he concludes that "we can but try". This, however, is a mere setting aside of the theoretical question whether such a thing *is* possible; there is no merit in stopping inquiry in case it should extinguish a particular hope. And, as regards the content of Flugel's hope, it, on the face of it, falsely divides man from nature and begs the question whether there can be any struggle with "the forces of nature" which is not also (or does not involve) a social struggle. Here, as elsewhere, the appeal would be to history, to social facts and not to hopes—or to the dialectic of Engels. And I should say that the appeal would have to be decided against Flugel, unless "human nature" is to be taken as indefinitely variable—unless, indeed, this is what his conception of Progress means—in which case there is no such thing as human nature or as human (psychological and social) science.

Something of the looseness of Flugel's writing will have been apparent from the quotations given above, but there are a number of points that call for special remark. There are such errors in English as "cannot help but", "compensate" in place of "compensate for" (more than once), the projection of something "on to" something else (repeatedly). There is the statement (p. 108) about the 'English School' that it "centres round

the pioneer work of Melanie Klein, who developed a play technique which enabled something resembling psycho-analytic treatment as employed with adults to be adapted to the use of very young children of from 2 to 6 years old"—where clearly the "something resembling" is not what is adapted but is the *outcome* of the adaptation in question. With this may be compared the statement (p. 226) that the system of magic and superstition "endeavours, as Freud showed, to prolong infantile 'omnipotence of thought' and is indeed mankind's most desperate and thoroughgoing attempt in this direction, actually seeking to convert wishful thinking into *something like an exact pseudo-science*", and the remark (p. 164), regarding the difficulty, in the present state of psycho-analytic knowledge, of accounting for or predicting reactions to punishment: "All that we can safely say is that, as often happens at a certain stage in the progress of scientific thought, *improved insight has revealed a somewhat bewildering confusion* of factors at work behind familiar phenomena" (my italics in both cases). The constant and irritating use of hedging or modifying expressions reaches its climax in the statements in three successive sentences (pp. 179, 180) that a certain strength "seems to emanate from the loved object", that the superego "seems, as we might be inclined to say, to embrace, attract, and elevate the ego", and that "in mania also the distinction between the ego and the super-ego seems in some way to be obliterated". Again, the spurious relationships "corresponds to", "represents", "reflects", and the vague "is connected with", are regularly introduced as if they were quite specific and important forms of connection. Finally, we may take the following as the best illustration of the personification referred to earlier: "guilt having been removed and the super-ego satisfied by suffering, the ego is free to turn a favourable ear to the solicitations of the id towards forms of gratification that would be unacceptable as long as guilt remained" (p. 159).

Heaviness of style is at least partly accounted for by heaviness of purpose—by the author's meliorism or sal-

vationism. But, concerned though it was with questions of cure, there was nothing in the original or "classical" doctrine of psycho-analysis which required the adoption of a voluntarist or salvationist view. What was striking about it was its objective and determinist treatment of mental facts, and such a treatment, while opposed to the more recent conception of a peculiar "psychic reality", would harmonise with an objective and determinist treatment of ethics. It would have been possible to give a much more detailed exposition and criticism than I have attempted here of Flugel's ethical relativism, particularly in the form of biologism. But this would have been only incidental to the issues on which I have concentrated—the objective character of ethics and the positive conditions of the working out of ethical theory.

John Anderson.

DISCUSSION

"GRAMMATICISM"

By Thomas McPherson

In his recent "Reflections" on Mr. Antony Flew's *Logic and Language* (First Series) Professor Passmore drew attention to the fact that there is no universally accepted name for the philosophical movement represented in that book.[1] It is called sometimes the linguistic movement in philosophy, sometimes linguistic philosophy, sometimes Wittgensteinian philosophy, sometimes other things. Passmore suggests the name "grammaticism".

I cannot think that this suggestion would be received with enthusiasm by members of the movement. If I give my reasons for saying this it may help to throw light on what the contemporary linguistic philosopher does additional to that shed by Passmore's excellent pages.

It is certainly true that some contemporaries describe themselves as interested in "questions of logical grammar", but the word that matters is not "grammar" but "logical". What does "grammaticism" suggest? It suggests what grammarians do. And what do grammarians do? A grammarian—to put it very generally—is someone whose job it is to state the rules followed by the speakers or writers of a language when they are speaking or writing the language "correctly".[2] (I do not know that one can clearly separate so-called normative or prescriptive grammar from descriptive grammar, as some would wish to do.) The traditional divisions of grammar are into phonology, accidence, word-formation, and syntax, and each of these is further subdivided. Now surely it is clear that information—or most sorts of informa-

[1] J. A. Passmore, "Reflections on *Logic and Language*", this *Journal*, December, 1952.

[2] *Cf.* J. Mackie, "The Logical Status of Grammar Rules", this *Journal*, December, 1949.

tion—about sounds, inflexions, word-order, etc., is not what the philosopher wants.

That the philosopher's interest in language is another sort of interest from that of the grammarian is seen in the fact that philosophers have for a long time been maintaining that, as they significantly say, grammatical likenesses can hide logical differences. It is is the looking for logical differences beneath grammatical likenesses (or logical likenesses beneath grammatical differences) that occupies the linguistic philosopher.

The clear realization of this is sometimes dated[3] from Russell's early writings—say, from 1900. But Russell was, of course, not the first philosopher to point out that grammatical likenesses can hide logical differences. Frege said it—or implied it—in 1884.[4] Bradley in 1883 had insisted on a distinction between the "real" subject and the grammatical subject of a judgment.[5] We can go back far beyond Bradley—to Coleridge, who, in 1819, used very modern-sounding language in pointing out that the Paradox of the Liar arises because of "bad grammar", but meaning by this bad grammar of a kind which constitutes a logical defect—"bad logical grammar".[6]

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[3] As it seems to be by Wittgenstein himself. See *Tractatus Logico-Philosophicus*, 4.0031.

[4] G. Frege, *Foundations of Arithmetic* (trans. Austin), pp. 39-41.

[5] F. H. Bradley, *Principles of Logic*, *passim*. In the First Terminal Essay, added in 1922, he remarks: "The mere grammatical form, I have pointed out, is very apt to mislead us. The real subject of our process can not be assumed to lie in that which makes the subject of our sentence"—p. 618.

[6] S. T. Coleridge, *Philosophical Lectures*, ed. Coburn, p. 199. Coleridge's solution of the paradox is an interesting one. He writes: "When a man says 'I lie', he either lies or he does not lie. If he lies, he tells the truth, and if he tells the truth, he lies—a sort of conundrum. Every man knows at once the thing must end in nothing, that it is an absurdity, and yet it will not suggest itself at once to every mind where the logical defect is, which in this case, for instance, would be that here a poetic or passionate use of a phrase is substituted for a logical one, that when a man says 'I lie' he uses the present tense figuratively, in order to determine the extreme recency of what he has said; and the sophism consists in the man having used bad grammar and saying 'I lie', which is referable to the past, as a positive act of immediate consciousness; for if he said 'I lied' the argument would drop".

The linguistic philosopher does not really think of himself as a grammarian (though Wittgenstein almost implies this in *Philosophische Untersuchungen*). And I do not imagine that Passmore seriously wants to maintain that the linguistic philosopher is a grammarian whether he thinks he is or not. Passmore must be well aware that it is logical differences and not grammatical differences that the philosopher is looking for in language. It is, then, unfortunate that he has chosen the name "grammaticism". In any case, if he wanted to stress particularly the interest of contemporary philosophers in showing up bad grammar, "although in a special sense of that phrase",[7] he would have done better to coin a completely new word—say, "grammatism". "Grammaticism" is not a particularly good word for the purpose Passmore wants to use it for. The *Oxford English Dictionary* defines it as "a point or principle of grammar; a grammatical definition". Passmore says he wants to give a new meaning to the word. He has indeed given it a new meaning. There is altogether too little in common between what the word normally means and what Passmore wants to make it mean for his use of it to be other than misleading. There seems little point—unless one first explains oneself—in using a word in a sense other than that it would normally be taken to have when one's intention is to be helpful. Altogether I should have thought that the expression "linguistic philosophy", which is perhaps that in most general use in discussions of this contemporary philosophical trend, would do well enough; and that Passmore need not have taken the trouble to think of a new name.

Passmore's suggested name for the linguistic movement is, then, misleading because it implies a wrong classification of the contemporary philosopher's activity; it suggests that it belongs with the activity of the grammarian. Both grammar and philosophy are themselves fundamentally sciences of classification, but philosophy is concerned with classification on a particularly high level; for among other questions

[7] Passmore, *loc cit.*, p. 153.

it is concerned with the question of how it is itself to be classified among the sciences. ("Sciences" in the wide sense.) I can best explain this notion of philosophy as a science of classification—as *the* science of classification—by shifting for the moment to another question.

I said above that the word to stress in the expression "logical grammar" that is used by some writers is not "grammar" but "logical". Many writers of this kind would claim that they are logicians, and they might even claim that the whole of philosophy reduces to logic. Now students are often puzzled by what "logic" means nowadays. There is, I think, a tendency to suppose that whereas now everything is complicated and confused the word "logic" *once* had a perfectly clear and agreed meaning. In fact, of course, the definition of "logic" has for some time been a thing on which there has not been general agreement. The various "schools" have differed widely, and a complete account of what the word "logic" means or has meant could not be given except at considerable length.[8] But, even so, nowadays, some would say, the word has got completely out of hand.

What is one to make of phrases like "the logic of personality", "the logic of research", "the logic of chess", "the logic of industrial organisation"?[9] And what in particular is one to make of the view that philosophy *is* logic? It is not only students who are puzzled about what the word "logic"

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[8] This is not to deny that within the "tradition" in logic there is what might be called an agreed definition.

[9] This last phrase is the title of a book by P. Sargant Florence published in 1933; re-issued in 1953 as *The Logic of British and American Industry, a Realistic Analysis of Industrial Structure and Government.* This example is interesting as showing that it is not only philosophers who use the expression "the logic of . . .". It is also interesting to note that Otto Jespersen announced in 1922 a forthcoming *The Logic of Grammar,* the well-known book that eventually appeared in 1924 under the title *The Philosophy of Grammar*; this is of some significance in connexion with the tendency in some quarters today to regard "logic" as (almost) synonymous with "philosophy"; even a non-philosopher—as we see—may be found doing this, though admittedly Jespersen may not have meant by either "logic" or "philosophy" what most philosophers mean by those words.

means nowadays. One professional philosopher recently began a review of Polanyi's *The Logic of Liberty* thus:

> "A popular gambit in current philosophical discussion is to talk about 'the logic of' some word or phrase. It is high time someone explained 'the logic of' this expression itself. Tweedledee's use of the word 'logic' is simple and clear: 'If it were so, it would be; but as it isn't, it ain't. That's logic.' But 'logic' acquires an aura of haze when we hear of 'logical geography' or 'plotting the logic of' some word or concept." [10]

Perhaps it *is* high time somebody explained the phrase "the logic of", and related expressions. I hope some day someone will. In the meantime here is a suggestion.

A convenient clue to the understanding of the contemporary use of "logic" may, I think, be found in the title of one of Peirce's papers: "How to Make Our Ideas Clear".[11] Logicians have always been concerned with the making of our ideas clear. The Theory of Terms and the Theory of Propositions in the traditional logic is really directed at the preliminary *clarification* of the elements of arguments, and the Theory of Inference is concerned with the *clarification* of the notion of valid argument. The kind of clarification involved here is, of course, *classification*—the looking for relevant differences in the material and the sorting of it out into types according to those differences.

Indeed, it is perhaps true that the chief way in which we go about making any ideas clear is by *classifying*: putting things into their right boxes and, in some ways more importantly (though it is fundamentally the same occupation), taking out things that have somehow got into the wrong boxes. Naturally, as always happens with classification, there may be a certain amount of distortion of the material (or overlooking of otherwise significant features in it) in order to make it fit a limited set of models. This need not be a bad thing; it is sometimes illuminating.

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[10] D. Daiches Raphael, in *The Philosophical Quarterly*, January, 1953, p. 86.

[11] In Vol. V of *The Collected Papers of Charles Sanders Peirce.*

Philosophers have always been concerned with finding the most appropriate classifications of things.[12] What is justice? What is the mind? the self? What are material objects? What is goodness? beauty? truth? The answers that philosophers have traditionally given to such questions have consisted essentially in putting justice, the self, goodness, or whatever it might be, into one box rather than another, with reasons why the suggested way of classifying is more appropriate than others which have been, or which might be, suggested.

So, "the logic of personality" is concerned with the proper classification of "facts" of personality, "the logic of chess" with the proper classification of moves in chess, and so on.

There is much more that ought to be said about philosophy as the science of classification. This is not the place to say it, but it is the place to enter a warning against taking too seriously my statement that philosophy is usefully to be regarded as *the* science of classification: this, like any other statement about what "all" philosophy is, may be as misleading as it is intolerant. One of the things the linguistic philosopher is most concerned to stress is the inadvisability of making too many generalisations, and I have made so many already that I had better stop.

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[12] *Cf.* G. Ryle, "Categories", in *Proc. Arist. Soc.*, 1938-9, or in Flew's *Logic and Language* (Second Series). And *cf.* also "Philosophy is the replacement of category-habits by category-disciplines" (Ryle, *The Concept of Mind*, p. 8).

NOTES AND NEWS

Australasian Association of Psychology and Philosophy Annual Congress, 1953

The Congress and Annual General Meeting for 1953 were held at Melbourne from August 21st to 26th. The programme was as follows:

Friday, August 21st:

8 p.m.—Presidential Address: Dr. W. D. Falk, "Duty and Self-Protection".

Saturday, August 22nd:

10 a.m.—Mr. J. L. Mackie, "Evil and Omnipotence".
2.30 p.m.—Council Meeting.
8 p.m.—Prof. A. K. Stout, "But suppose everybody did the same".

Sunday, August 23rd:

8 p.m.—Mr. C. F. Presley, "Laws and Theories in the Physical Sciences".

Monday, August 24th:

10 a.m.—Dr. K. Baier, "The Point of View of Morality".
2.15 p.m.—Annual General Meeting.
3 p.m.—Discussion.

Tuesday, August 25th:

10 a.m.—Symposium, Prof. J. J. C. Smart and Prof. D. A. T. Gasking, "Mathematical Logic and Philosophy of Mathematics".
3 p.m.—Discussion.
8 p.m.—Mr. Q. B. Gibson, "Objectivity in Argument".

Wednesday, August 26th:

10 a.m.: Prof. D. Taylor, "Thinking".
3 p.m.—Discussion.